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Gender-targeted Conditional Cash Transfers

Enrollment, Spillover Effects and Instructional Quality

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Abstract

This paper considers the effects of a gender-targeted conditional cash transfer program for girls in classes 6 to 8. It finds that the program is successful in increasing the enrollment of girls in classes 6 to 8 as intended. It also finds evidence to suggest that the program generated positive spillover effects on the enrollment of boys. This success does, however, appear to be poised to come at a cost. The student-teacher ratio in treated districts is also climbing. This suggests that in the absence of active steps to address these increasing student-teacher ratios, instructional quality is likely to suffer. The success of the program appears to be driven by enrollment increases in urban schools. This suggests the need for a reassessment of the targeting criteria in rural schools.

This paper—a product of the Poverty and Gender Group, Poverty Reduction and Economic Management Sector of the Latin America and Caribbean Region—is part of a larger effort in the department to evaluate the evaluate the impact of social programs. Policy Research Working Papers are also posted on the Web at http://econ.worldbank.org. The author may be contacted at ahasan1@worldbank.org.

Gender-targeted conditional cash transfers: Enrollment, Spillover Effects and Instructional Quality

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1. Introduction

Conditional cash transfer programs (CCTs) have become increasingly popular anti-poverty instruments in the developing world. Spurred on by the well-documented success of programs such as *Oportunidades* (formerly *Progresa*) in Mexico, policymakers around the world have adopted, adapted and scaled up conditional cash transfer programs in countries as distinct as Cambodia and Pakistan. These newer programs all share a basic feature of the original transfer schemes: they provide cash incentives for investments in the health and education of children. However, they vary on several other dimensions.

CCT programs can be roughly categorized into those that support investments in education or health or both. Program eligibility and the size of the transfer (τ) can vary according to the characteristics (j) and gender (k) of the child. Often these characteristics are the ages of the children or the grades in which they are enrolled. Thus boys and girls may receive different-sized transfers and this may depend on how old they are or which grade they are enrolled in:

$$\tau_j^k = \begin{cases} [0, \beta(j)] \; if \; k = boy \\ \\ [0, \gamma(j)] \; if \; k = girl \end{cases}$$

While *Oportunidades* / *Progresa* had a strong evaluation component built into its design other programs have not taken a similarly holistic view of program design and implementation. Consequently while policymakers have been adept and innovative at fine tuning program rules, often they have not required the collection of quality data. This has meant that analysts have been hampered in their attempts at evaluating these programs by governments who do not collect data or worse yet agencies that do not release data already collected. Despite the large and consistent body of evidence on the beneficial effects of various CCT programs, we are only now beginning to understand the role various program features play in producing results.

This paper focuses on the use of gender-targeting to deliver a conditional cash transfer program. This analysis extends our understanding of conditional cash transfers in the following ways: (1) by analyzing data from the entire lifetime of the program, I am able to estimate the full history of the effects of the program on enrollment. I analyze male and female enrollment separately to test if there are any spillover effects on boys' enrollment of a program that was targeted only to girls. (2) I examine the student teacher ratio as an outcome to determine whether, in the long run, the instructional quality at schools in treated districts is adversely affected. (3) I investigate the existence of heterogeneous treatment effects by examining whether schools in urban or rural areas are more successful at enrolling girls.

I provide a brief overview of the relevant literature in section 2 before describing the program in section 3 and the available data in section 4. In the sections that follow I detail the identification assumptions inherent in the analyses I perform (Section 5), and provide descriptive evidence (Section 6). Section 7 details the empirical specification and describes the results. In Section 8 I subject the results to a series of robustness checks. Section 9 concludes with a discussion of the relevance of these findings for policy.

2. Literature Review

The prototypical CCT program is Mexico's *Oportunidades* program (formerly *Progresa*). In part, the literature focuses on this program because it was implemented as a randomized experiment and detailed baseline and follow-up data were collected on both treatment and control group villages. Consequently there exist a host of evaluations of *Progresa* which utilize data from the randomized experiment and supplement their findings with results from regression-adjusted difference-in-difference estimators. Parker et al. (2008) provide an up-to-date and in-depth review of the state of the literature. Therefore, in this paper I provide only a brief overview of the strand of the literature relevant to the current analysis and then turn to program details.

The *Progresa* program has been evaluated on a range of outcomes: enrollment (Schultz, 2000 and 2004), labor force participation (Skoufias and Parker, 2001), morbidity, height, prevalence of anemia (Gertler, 2004), food consumption (Hoddinott and Skoufias, 2004), intra-household allocation of expenditure (Martinelli and Parker, 2008), and investment behavior (Gertler et al. 2006). The effectiveness of *Progresa* is well-documented along virtually all of these dimensions. Subsequent evaluations on the schooling effects of Progresa have sought to identify how the effects of the program vary: via endogenous peer effects (Bobonis and Finan 2006), via within family effects (Parker et al. 2005) and family network effects (Angelucci et al. 2008).

What is absent from these evaluations is an examination of the role school location and infrastructure might play in increasing enrollment. Do schools with better infrastructure register larger increases in enrollment than do schools without such infrastructure? Are urban schools more easily able to attract students than rural ones? Maluccio and Flores (2004) find increases in food expenditures, school enrollment, nutritional status and participation in health care in their evaluation of Nicaragua’s *Red de Protección Social* program. When this program was shut down because it was too expensive an examination of any long run effects was ruled out. This begs the question, do CCT programs sustain their early successes?

Only a few conditional cash transfer programs are explicitly targeted to girls. Of particular relevance to the current analysis are the programs in Cambodia, Bangladesh and Pakistan. In each of these countries, policymakers have explicitly targeted the transfer on the basis of gender – providing transfers to girls only. Using repeated cross-sectional data, Khandker et al. (2003) find that the Bangladesh Female Secondary School Assistance Program led to a substantial increase in the enrollment of girls and found no spillover effect for boys. Evidence on spillover effects from other programs is limited. Hasan (2009) examines the effects of a gender-targeted CCT program on household time allocation. Chaudhury and Parajuli (2006; 2008) use school censuses in conjunction with a district-level eligibility cut-off and find that Pakistan's female student stipend program in Punjab between 2003 and 2005 increased enrollment by six female students per school (a nine percent increase in female enrollment in terms of relative change). Filmer and Schady (2006), exploiting the cut-off score used to determine eligibility for Cambodia's Japan Fund for Poverty Reduction (JFPR) scholarship, find a 30 to 43 percentage point increase in the enrollment and attendance of girls.[1] Papers that explicitly examine how program design features influence outcomes are more recent: for example, Filmer and Schady (2009a) find evidence of diminishing returns to the size of the transfer.[2]

Consequently, this paper has a modest goal: to build on this earlier work and to provide a more complete picture of the effects of a gender-targeted conditional cash transfer program on outcomes of interest to policymakers: girls' enrollment and boys' enrollment. I consider whether school infrastructure or location mediates the effects of the program. Lastly I examine whether

[1] For additional analyses using the Cambodia data see Filmer and Schady (2009b) and Ferreira, Filmer and Schady (2009).
[2] See also de Janvry and Sadoulet, (2006.)

instructional quality is affected by testing whether the student teacher ratio increases when class sizes increase through the introduction of CCT programs.

3. Program Description

As part of the government of Pakistan's commitment to the reduction of gender disparity in education, it initiated a conditional cash transfer program targeted to female students in Punjab. Under the program girls receive a stipend of Rs. 200 per month (approximately US$ 3.5) if they are enrolled in grades 6 to 8 in a government school and maintain a minimum attendance of 80 percent.[3] The stipend is paid in quarterly payments to the student via a postal money order. To give this amount context, consider that a day's unskilled wage for a male in these areas is estimated to be Rs. 60. Thus an unskilled male who works 30 days a month can expect to receive Rs. 1800 a month. The stipend, then, amounts to one-ninth of the average monthly income for an unskilled worker.

The program was announced in 2003. The government did not consider randomization politically feasible and therefore used district-level literacy rates for the population aged 10 and over according to the 1998 census (the most recent census at the time) to determine which districts would be eligible to receive the stipend program. Districts with literacy levels below 40 percent were deemed most in need and hence were eligible for receipt of the stipend program. Those districts with literacy rates higher than 40 percent were deemed ineligible. Implementation of the program reportedly did not begin in full until the second quarter of 2004. Figure 1 reports the literacy rates of the treatment and comparison districts on a spatial map, allowing the reader

[3] Anecdotal evidence suggests that the 80% attendance requirement is not strictly enforced and that enrollment is a more salient condition.

to get a sense of where in the province the stipend program was active. Figure 2 condenses this information and distinguishes between treated and comparison districts directly.

4. Data

This paper utilizes data from provincial school censuses made available by the Project Management Implementation Unit (PMIU) of the Punjab Education Sector Reform Program (PESRP). School censuses have been conducted annually since 2003 (the baseline year). This paper uses all the rounds from 2003 to 2008. Each school is assigned a unique eight-digit code in the Educational Management Information System (EMIS) which allows schools to be tracked from year to year.

Each census covers approximately 60,000 government schools in the province of Punjab. More than 40 percent of these schools are primary schools. While they are, strictly speaking, only meant to enroll students in primary school grades (classes 1 to 5), some schools do report students in typically middle school grades (classes 6 to 8). Schools report whether or not they are functional and what their designated level is – whether they are a primary or middle school and whether they are a high school or a higher secondary school.[4] In addition information is provided on whether the school is urban or rural and whether the school is for boys or for girls or in some instances for both. The survey instrument asks about the type of material used in construction of the school building, its state of repair, its ownership status, the year the school was established and how much covered and uncovered area is occupied by the school.

Each year the census form requires schools to declare the number of boys and girls enrolled in each class as of the start of the school year in September/October. These enrollment

[4] A functional school is one that is open and operating. Schools may not be open or may not operate if there is a lack of teachers – hence the distinction between functional and non-functional schools.

figures are reported separately for boys and girls and for each grade from one to twelve.[5] In addition to identifying the district where a given school is located, the census asks a series of questions about the infrastructure of the school. These questions ask whether a school has each of the following amenities: a toilet, drinking water, electricity, a main gate, a boundary wall, a play ground, a library, laboratory facilities for physics, chemistry, biology, home economics and computers. Lastly for each school the analyst is provided with information on the number of teaching positions sanctioned by the educational authorities and the number of teaching positions that are currently filled.

In the main analysis that follows, schools form the unit of analysis. There are three main dependent variables of interest: girls' enrollment, boys' enrollment, and the ratio of filled teaching positions to sanctioned teaching positions. The availability of longitudinal data from two groups (schools in untreated districts and schools in treated districts) suggests a difference-in-difference approach.[6]

5. Identification Strategy

Consider the stylized example depicted in Figure 3. Data are available on an outcome (Y) for schools from two sets of districts– those that receive the stipend treatment ($Y_{D=1}$) and those that do not ($Y_{D=0}$). The vertical dashed line at time t represents the implementation of the program. Consequently t-1 is the baseline year (2003) and t+1 is the post-program period (2004 to 2008). Outcomes for two groups are shown: the control group (solid grey line) and the treated

[5] In addition there are two sets of students for whom enrollment is also reported but who do not constitute a formal part of the school's student body – un-admitted students and students in the so-called *katchi* (nursery) class.
[6] The application of a strict district-level literacy cutoff in determining program eligibility suggests that the data could also be analyzed using a sharp regression discontinuity design (RDD). In the interests of space and because these estimates have limited external validity, I focus on the cutoff for descriptive analyses only in Figures 12 and 13.

group (solid black line). The dotted line CD represents no change in the outcome of interest over time. The line CEF represents the evolution of the outcome for the treatment group over time in the absence of any program. Consequently, the distance FD is the natural trend in the outcome over time that would exist in the absence of any program. The line AB shows how the outcome for the comparison group evolved over this time frame. Without such information, the effect of the program would be overstated. The difference between the two groups at baseline (the distance AC) is assumed to have persisted (the distance BF is drawn to equal the distance AC). However, the implementation of policy causes the outcome for the treated group to evolve differently. Consequently, the outcome for the treated group follows the path GH after the program is implemented. The difference-in-differences methodology allows the decomposition of outcomes over time into a part due to a natural trend over time (FD), a part due to pre-existing differences between the two groups (BF) and the part that is due to the program (HF).

The data reveal that at baseline (before the program was underway) those districts with a literacy level higher than 40 percent had (not surprisingly) larger enrollments for both girls and boys for all the grades reported in Table 1. The schools in districts with no stipend program provide a picture of the typical pattern of enrollment in the absence of the program. The characteristics of these schools are reported in Table 2.[7] In the context of Figure 3, they provide the information necessary to draw the line AB. By contrast in the districts where the stipend program is available, schools provide the information necessary to draw lines CE and GH.

[7] These characteristics are by and large similar to those in Table 1 and are reported for completeness as they will aid in interpreting the regression coefficients reported in subsequent tables.

6. Descriptive Evidence

The most basic analysis proceeds from a comparison between treatment and control districts of the average enrollments in various classes over the course of the program. These enrollment figures are plotted in Figures 4 through 7 for girls in classes 5 through 8 respectively. Of these classes 6 to 8 are eligible classes under the stipend program. The enrollment data on girls in class 5 serves as a placebo test in the absence of strategic choices by parents. Since the stipend is only for classes 6 to 8 there should be no effect of the program on enrollment in class 5. I plot the averages for treatment and control districts over time and along with their 95% confidence intervals. At baseline there are differences in each grade as would be expected. Over time the confidence intervals begin to overlap suggesting that there is a change in enrollments for the treatment districts. Figures 4 through 7 suggest that the program increased girls' enrollment not only in classes 6 to 8 but also somewhat in class 5.

Similarly since the program is only for girls attending classes 6 to 8, there should be no change in boys' enrollment. In the absence of spillover effects, the enrollment of boys in grades 6 to 8 serves as a placebo test. Figures 8 through 10 reveal that the enrollment of boys in both treatment and control groups follows a similar pattern for classes 5 to 7. For boys' enrollment in class 8 (Figure 11) there is slight evidence of a spillover effect as the enrollment of boys in the treatment district appears to catch up to those in the control districts. Taken together the evidence in Figures 8 through 11 suggests some evidence of a spillover effect due to the stipend treatment.

Figures 12 and 13 present district level aggregates for the eligible classes and exploit the government's use of the literacy rate cut-off to differentiate treated and untreated districts from each other. In figure 12 I order treated districts by the literacy rate (the so-called forcing variable in terms of RDD nomenclature) and plot the total number of girls enrolled in classes 6 to 8. Each panel represents a different year of the data. These display the progression of how the program increased enrollment year by year. In figure 13 I repeat this graphical analysis using boys' enrollment in the same grades. At the level of descriptive statistics, these figures reveal much the same pattern as those above: girls' enrollment increased over time as a result of the program and generated a positive spillover on boys' enrollment as well.

7. Empirical Specification

In order to see if these trends are robust to the inclusion of controls, I estimate a difference-in-differences model and apply the following empirical specification:

$$Y_{it} = \alpha_0 + \alpha_1 I[D=1] + \sum_{j=2004}^{2008} \alpha_j I[Year=j] + \sum_{j=2004}^{2008} \delta_j I[Year=j] * I[D=1] + \sum_{l=1}^{L} \beta_l X_{it} + b_i + u_{it} \qquad (1)$$

Where Y_{it} is the enrollment in school i at time t in a given class (5 to 8), the vector X_{it} is a set of time-varying observable characteristics of the school, the terms $I[Year = j]$ are indicator variables for each year j except the baseline and the term $I[D = 1]$ is an indicator variable that equals one if the school is in a treatment district. The coefficient δ_{2004} captures the effect of the program one year after program began and the coefficient δ_{2005} captures the effect of the program two years after program implementation. Similarly, the coefficients δ_{2006}, δ_{2007} and δ_{2008} represent the effect of the program three, four and five years after the program began respectively. The term b_i reflects a school-level individual fixed effect and incorporates the influence of time-invariant regressors – measured and unmeasured – on enrollment and u_{it} is an error term. Basic descriptive statistics of the data are reported in Tables 1 and 2.

7.1. Results: Girls' Enrollment

In Table 3 I recreate the estimates presented in Chaudhury and Parajuli (2006; 2008): a first difference approach that uses only two years of data – 2003 and 2005. This serves two purposes: (1) to ensure that the data released by the government to both sets of authors is the same and that comparable decisions were taken in cleaning and setting up the data for analysis and (2) it highlights the fact that using boys' enrollment as a counterfactual is an unwise choice in the context of this gender-targeted program.

In particular, note that the estimates in Table 3 suggest that the average school in the treated district experiences an increase of 11 girl students in classes 6 to 8 when compared to the average school in the control district. Similarly the average school in the treated district experiences an increase of 10 boy students in classes 6 to 8 when compared to the average school in the control district. From Table 2, in 2003, a typical school in the treated district has roughly 42 girl students in classes 6 to 8 and roughly 77 boy students. Thus the point estimates from first differencing (without controls) suggest a 26 percent increase in girls' enrollment and a 13 percent increase in boys' enrollment.[8] Given this evidence that is suggestive of possible spillover effects, I choose not to perform a so-called triple difference analysis with boys as a counterfactual.[9]

Also the choice of an aggregate dependent variable masks potential variation in outcomes that may exist by class. Consequently in Table 4, I examine the enrollment of girls in class 5. This is not an eligible grade under the program. However, if households anticipate that the

[8] The first percentage is calculated as $\left(\frac{11}{42}\right) * 100$ and the second is calculated as $\left(\frac{10}{77}\right) * 100$

[9] Strictly speaking I choose not to use the difference in the evolution of boys' enrollment in the treatment district compared to that in the control districts.

program will continue and realize that the only way to receive the stipend in class 6 is having their child complete class 5, then there will be evidence of a rise in enrollment in this class as well. Column 1 reports estimates from ordinary least squares (OLS). This pools the longitudinal data and ignores the existence of repeated observations on the same schools. Column 2 explicitly recognizes the presence of these repeated observations and reports estimates from a school-level fixed effects regression. Standard errors are robust and allow for the clustering of observations at the school level. Column 3 adds a series of indicator variables for whether a school has one, two or all three of the following basic facilities available: drinking water, electricity and toilets. This analysis suggests that when one compares all the schools in the treatment districts with all the schools in the control districts (irrespective of their level – primary, middle or secondary) there was an increase in class 5 enrollment in the treatment district that exceeded the class 5 enrollment gain in the control district. However the point estimates in columns 1-3 are practically small. The range of estimates goes from 0.165 students in 2004 to none in 2005 to 0.824 students in 2008. However, when one excludes primary and mosque schools from the analysis a clearer picture emerges. In 2004 and 2005, the first two years of the program, there is no increase in class 5 enrollment in those schools offering up to middle and high school grades. In 2005, the program was expanded to include classes 9 and 10. It would seem that relative to 2003 in the period 2006 to 2008, the average middle and high schools in the treatment district experienced an increase in class 5 enrollments of 1 to 2 students each year more than their counterparts in the control districts. In terms of baseline enrollment these are modest enrollment gains – 10 to 16 percent.[10] They do however suggest that as authorities signaled the continuity of

[10] The mean enrollment at baseline in middle and high schools in the treatment district is roughly 10 students. I divide the point estimate in column 5 of table 4 by this number and multiply by 100.

the CCT program people whose children were in schools that offered classes 6 to 8 altered their behavior early to be able to benefit from the program down the road.

I turn now to the classes eligible for the program – classes six to eight. Each class is covered separately in Tables 5, 6 and 7, respectively. As before, column 1 reports OLS estimates, column 2 reports school-level fixed effects estimates and column 3 introduces controls. In Table 5 I find evidence to suggest that there was a one-year lag in responsiveness to the program. There is practically and statistically speaking no difference between 2004 and 2003 when comparing class 6 enrollments in the treatment and control districts. When comparing the point estimates for 2005 to 2008, I find evidence that the average school in the treatment district experienced a two-student gain in enrollment compared to the average school in the comparison district in 2005 and 2006. This grew to a three-and-a-half student gain in 2007 and 2008. In relative terms, combining this information with the information in Table 2, suggests that the enrollment increase ranged from a low of 12 percent in 2005 to a high of 23 percent in 2007.

Enrollment in class 7 (Table 6) shows a similar pattern with estimates ranging from an 11 percent increase in relative enrollment in 2004 to a high of a 30 percent increase in enrollment in 2007. Likewise for girls' enrollment in grade 8 (Table 7) the point estimates in column 3 suggest a range of program impact from a low of 9 percent in 2004 to a high of 33 percent in 2007.

7.2. Results: Boys' Enrollment

I noted at the outset that boys' enrollments in the treatment districts seemed to be growing at a faster rate than boys' enrollments in the control districts. This was based on data from one post-program year and for all three targeted classes (6 to 8) taken together. I present in Table 8 a closer examination of the evolution of boys' enrollment over time and consider classes

5 to 8 in turn. Each column corresponds to a different grade. I find evidence to suggest that there might exist a spillover effect of this gender-targeted conditional cash transfer program on boys' enrollment. Enrollments increase for boys in the treated districts in all the classes reported in Table 8. In percentage terms these gains are largest for boys in class 8 when comparing 2008 to 2004. To be sure there are gains made by boys in all classes for virtually each year of data available. However the estimates vary and are largest for more recent years.

7.3 Results: Instructional Quality

With substantial increases in enrollments for both boys and girls, the logical question to ask is whether quality of instruction is suffering as a result of increasing class size? In the absence of test score data for these students, I proxy for instructional quality using the student-teacher ratio. In this regard the detailed nature of the data is particularly useful. In developing countries in general and in Pakistan in particular, there is often a disparity between the number of teachers a school ought to have and the number of teachers it does have. This is particularly true in hard to reach areas where the positions may have been sanctioned but no suitable or available candidates have filled them. The school census form asks schools to report the number of teaching posts that are filled as of the time of the survey and the number of positions that have been sanctioned. I construct two measures of the student teacher ratio. One uses the number of sanctioned teachers as the denominator and the other uses the actual number of posts filled. This then provides us with an actual student-teacher ratio and an ideal student-teacher ratio (at least as envisioned by the sanctioning authority). The means of these variables are reported in Table 2 for both the average school in the treatment district and the average school in the control district. There is some evidence to suggest that the actual student teacher ratio (regression estimates are presented in Table 9) in practice is higher than what authorities would ideally like to have (regression estimates are presented in Table 10). The average actual student teacher ratio is 34 in a school in the treatment district. The coefficient on δ_{2004} is 1.71. This suggests a 5 percent increase in the student teacher ratio from 2003 to 2004. It is imperative to remember that an increase in the student teacher ratio, holding all else constant, is likely going to cause instructional quality to fall. The fact that this is not as large an increase as the increase in enrollment figures reported in earlier sections suggests that some attempts are being made on the ground to ensure that the instructional quality does not falter. By 2008 the increase in the student teacher ratio is approximately 16 percent. This is still half as large in percentage terms as the increase in student enrollment.

Table 10 considers the ideal student teacher ratio by changing the denominator in the dependent variable. This analysis provides us with some insight into how, if at all, policymakers are revising the number of sanctioned positions per school. Focusing our attention on the estimates in column 3 suggests that policymakers may be underestimating the need for sanctioned teachers. The numbers reported in column 3 show that the ratio of students to the sanctioned number of teaching posts also increases over time. In 2004, the average school in the treatment district experiences a 7 percent increase in the student teacher ratio when compared to the average school in the control district. By 2008 the increase when compared to 2003 is more than twice that at 16 percent. It is however, still lower than the enrollment increases discussed above. In the absence of test score data or other data on the quality of instruction, these two pieces of evidence would appear to suggest that while enrollment has gone up so too have student-teacher ratios. This may be adversely impacting the quality of the instruction taking place.

Table 11 takes into consideration the evidence presented above that boys' enrollment has been going up. I consider separately the student-teacher ratios in boys' and girls' schools – again distinguishing between the sanctioned number of teaching posts and those actually filled. The results indicate that the government is attempting to be reactive to the success of the policy by increasing the sanctioned number of teachers in girls' schools. It appears to be experiencing issues of capacity since the student-teacher ratio actually observed is going up by more than the sanctioned student-teacher ratio in these schools. In contrast, there is little evidence to suggest that the sanctioned student-teacher ratio at boys' schools changes at all over the time frame for which data exists. However, the observed student-teacher ratio in boys' schools is increasing. This imbalance must be explicitly addressed if the stipend program is to avoid inadvertently harming the learning opportunities of boys.

7.4. Results: Heterogeneous Treatment Effects

There is little reason to believe that the effects reported above are the same for all the units in our data. The most obvious dimension along which one might expect different program effects are school location and infrastructure. Thus the question becomes do urban and rural schools behave differently. If so, in what way? This question is addressed in Tables 12 and 13. Table 12 re-runs the difference-in-differences analysis on the subsample of rural schools in the data. Table 13 repeats the analysis using the subsample of urban schools in the data. At first glance it is readily apparent there are large differences in the size of the point estimates for urban and rural schools. Urban schools appear to be the driving force behind the results described above. Rural schools in the treatment districts do improve but by nowhere near as much as their urban counterparts. In order to test whether these coefficients are statistically different from each other, I perform a Hausman test.

Denote the coefficient δ_{2004} for the rural subsample as δ^{r}_{2004}. Denote the corresponding coefficient from the urban subsample as δ^{u}_{2004}.

The null hypothesis of this test is:

$$H_0: \delta^{r}_{2004} = \delta^{u}_{2004}$$

The test statistic follows the T-distribution and is given by:

$$T_{2004} = \frac{\delta^{r}_{2004} - \delta^{u}_{2004}}{\sqrt{\left(SE_{\delta^{r}_{2004}}\right)^2 + \left(SE_{\delta^{u}_{2004}}\right)^2}} \tag{2}$$

In equation (2) SE denotes the standard error of the estimate and I assume the covariance between the two estimates to be zero as they are from different samples.

Repeated application of this formula for each pair of coefficients from the difference-in-differences analysis reveals that in 17 of the possible 20 comparisons the t-statistic I obtain suggests that I reject the null hypothesis at the 95 percent confidence level or better.[11]

[11]

Tests of equality of coefficients: Girls' Enrollment in Rural v Urban

	Grade 5	Reject H_0?	**Grade 6**	Reject H_0?	**Grade 7**	Reject H_0?	**Grade 8**	Reject H_0?
T_{2004}	-2.59	Y	-3.10	Y	-2.12	Y	-1.95	N
T_{2005}	-1.74	N	-5.82	Y	-3.75	Y	-2.56	Y
T_{2006}	-2.73	Y	-5.89	Y	-6.86	Y	-4.64	Y
T_{2007}	-2.14	Y	-6.61	Y	-6.94	Y	-6.98	Y
T_{2008}	-1.44	N	-5.86	Y	-6.19	Y	-6.16	Y

8. Robustness Checks

I assess the robustness of the estimates in columns 4 and 5 of each of the preceding tables (Table 4 to Table 7) by limiting the sample of schools included in the analysis. I first exclude any mosque schools and any schools that are designated as primary schools (column 4). Mosque schools likely do not follow the typical grade progression associated with the program and primary schools (officially at least) are only able to offer instruction up to grade 5. Thus any students enrolled in higher grades in these schools may not be eligible to receive the stipend from the government. In column 5 I take advantage of access to program documentation from the PMIU which explains that two districts though initially assigned to treatment and control status respectively, subsequently had their treatment status interchanged. Specifically, when the program went underway in 2004, Multan was assigned to treatment while Khanewal was assigned to control status. This was done despite the fact that the district literacy figures from the 1998 Census would suggest that Multan was a candidate for control status while Khanewal should have been treated.[12] These restrictions do not make a substantive difference to the results detailed above.

9. Discussion

This paper finds that a gender-targeted CCT has substantial effects on enrollment. I find that not only does this program appear to increase the enrollment levels of the girls in classes 6 to 8 – the targeted group – but that the enrollment levels of otherwise ineligible boys in similar grades also appear to go up. It is possible that when girls are sent to schools their siblings are sent as well in order to accompany them to school or because the receipt of the stipend frees up enough resources to allow the household to increase the number of boys sent to school. It could

[12] This determination was made on the basis of a Multiple Indicator Cluster Survey (MICS) carried out at baseline.

also be that households that are induced to send their girls to school are unwilling to do so without also sending their sons. While the school census data used in this analysis can say nothing about spillovers within the household, a companion piece (Hasan, 2009) explores the effects of this program on time allocation within households using household survey data.

I take a holistic approach to the policy process and consider if the success of the program is able to shed any light on pitfalls policymakers must prepare for down the road. The school census data allow an examination of the student teacher ratio which suggests that while the student teacher ratio is increasing it is not increasing at the same rate as enrollment. This is precious little solace however if it indicates that things are not as bad as they could be. Policymakers must pay particular attention to the bottlenecks in the learning process that might appear if a business-as-usual approach is taken to staffing decisions. The CCT program is showing consistent evidence of being successful in terms of its stated goal of increasing girls' enrollment. However, this means that adjustments must be made to ensure that the students who are enrolling in school are also learning. That enrollment and learning are often divorced from each other is a point that has been repeatedly underscored in the literature on education.[13]

Likewise I consider whether schools from urban areas are better at producing enrollment gains. The evidence I find suggests that urban schools are driving the gains to the program. Rural schools are contributing to the success of the program but not by nearly as much as their urban counterparts. This goes to the very heart of the rationale for this program. The program documents state that the size of the stipend is intended to cover transport costs for girls. Transport costs may not be the right metric for assessing the size of the transfer in rural areas if the issue is ability to attend (access) rather than ability to get to school (transport). How

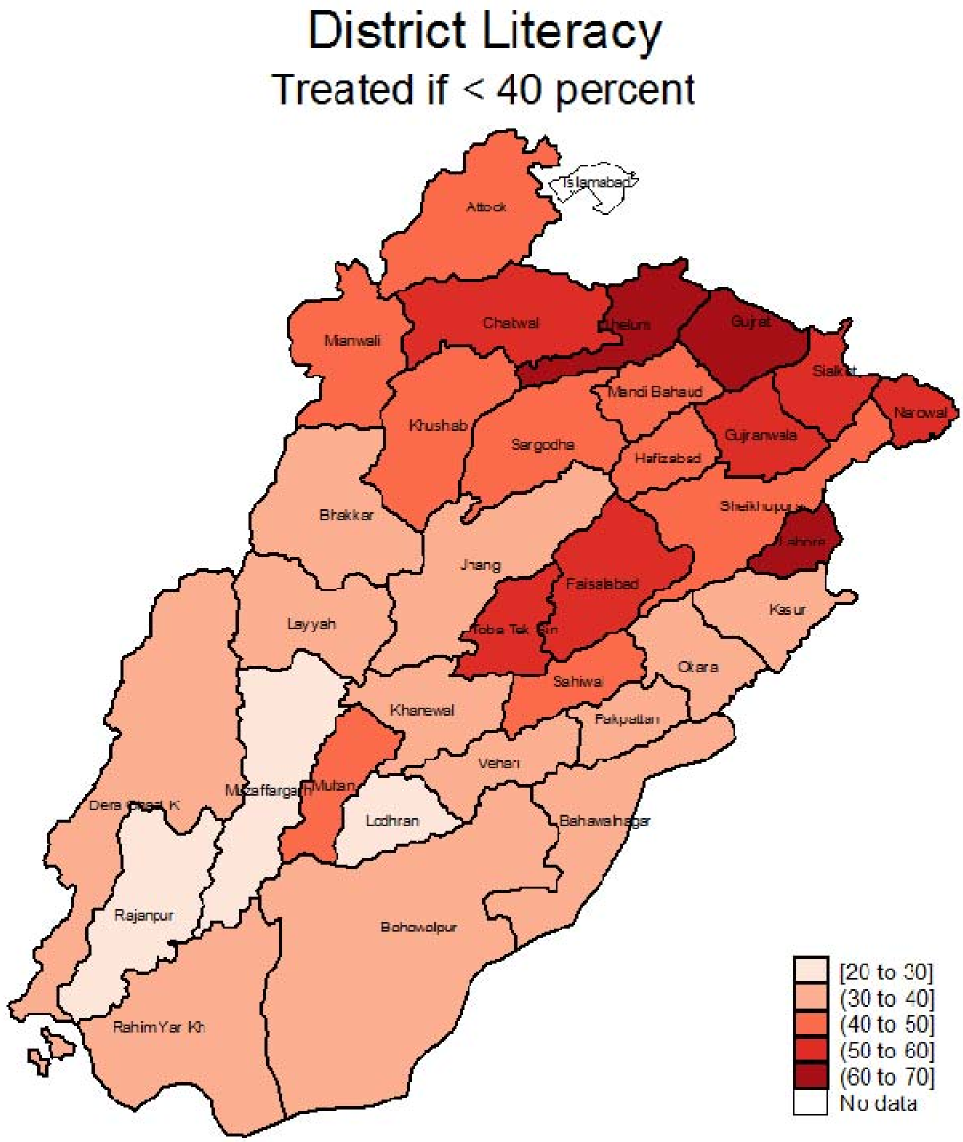
[13] See for instance, Filmer, Hasan and Pritchett, 2006 and the references therein.

successful the stipend program can be is inexorably linked to the state of infrastructure available for girls' education.

The evidence presented in Figures 14 – 16 suggests that this may indeed be the case. Figure 14 shows that in almost all parts of the province, at the primary level girls' school are generally half of all schools available. At the middle school level (Figure 15) however, there is much more variability. In a number of districts girls' schools account for as little as one-third of all schools. At the level of high school (Figure 16) the picture is bleaker with no district able to document gender parity in the number of schools. This state of affairs feeds back into the problem of having teachers to teach in these schools. If there aren't enough girls' schools at higher levels of education where are the female teachers of tomorrow going to get their education? This suggests one reason why rural schools might lag behind their urban counterparts – a dearth of well-qualified teachers in rural areas.[14] As the program continues and expands it will need to confront these issues to maintain the success it has already documented.

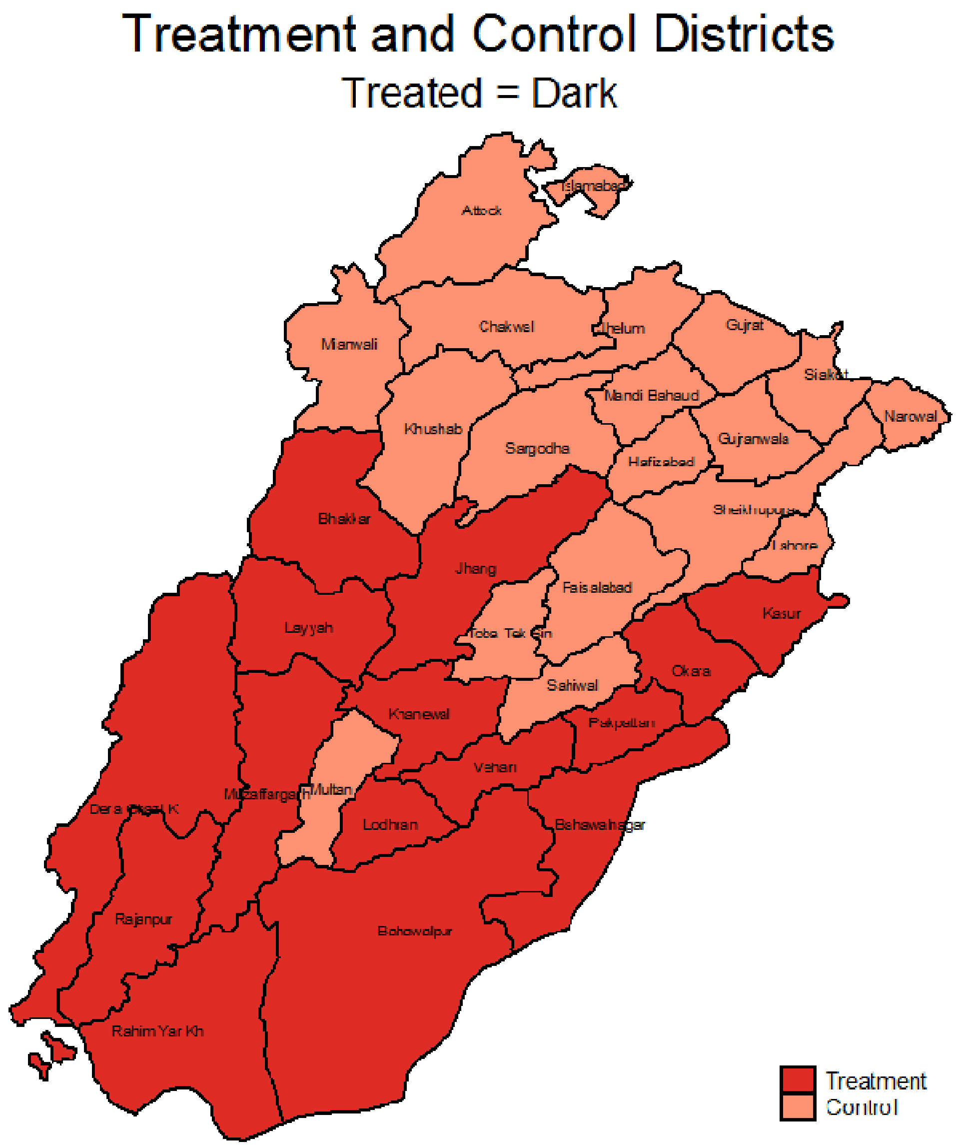
[14] In results not reported in this paper (for the sake of brevity) it appears that a simple index of facilities does not differentiate schools in their ability to increase enrollments under the CCT program. I am currently engaged in gaining access to information from the census forms on teacher qualifications. This might shed more light on how urban schools are able to consistently out-perform their rural counterparts.

Figure 1: District Literacy Rates



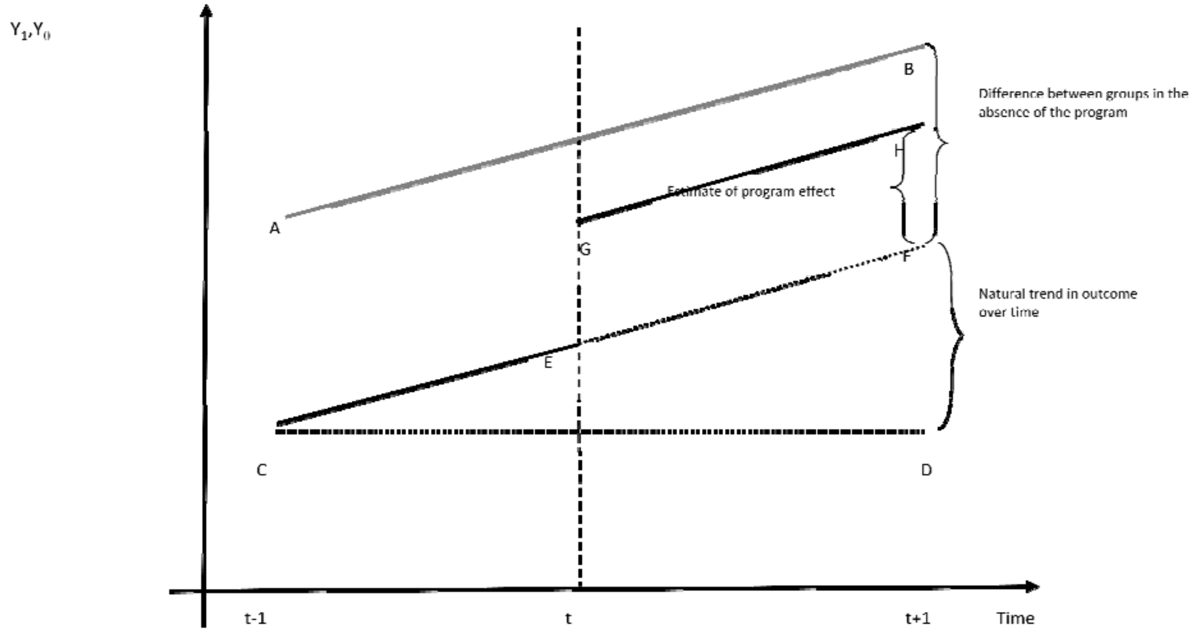
Note: Author’s calculations using PMIU-PESRP program documentation. District map of Punjab province. Districts where more than 40 percent of the population aged 10 and over was estimated to be literate at the time of the 1998 Census were deemed ineligible for the stipend program. Those where less than 40 percent of the population aged 10 and over was literate were deemed eligible to receive the stipend program.

Figure 2: Treatment and Control Districts



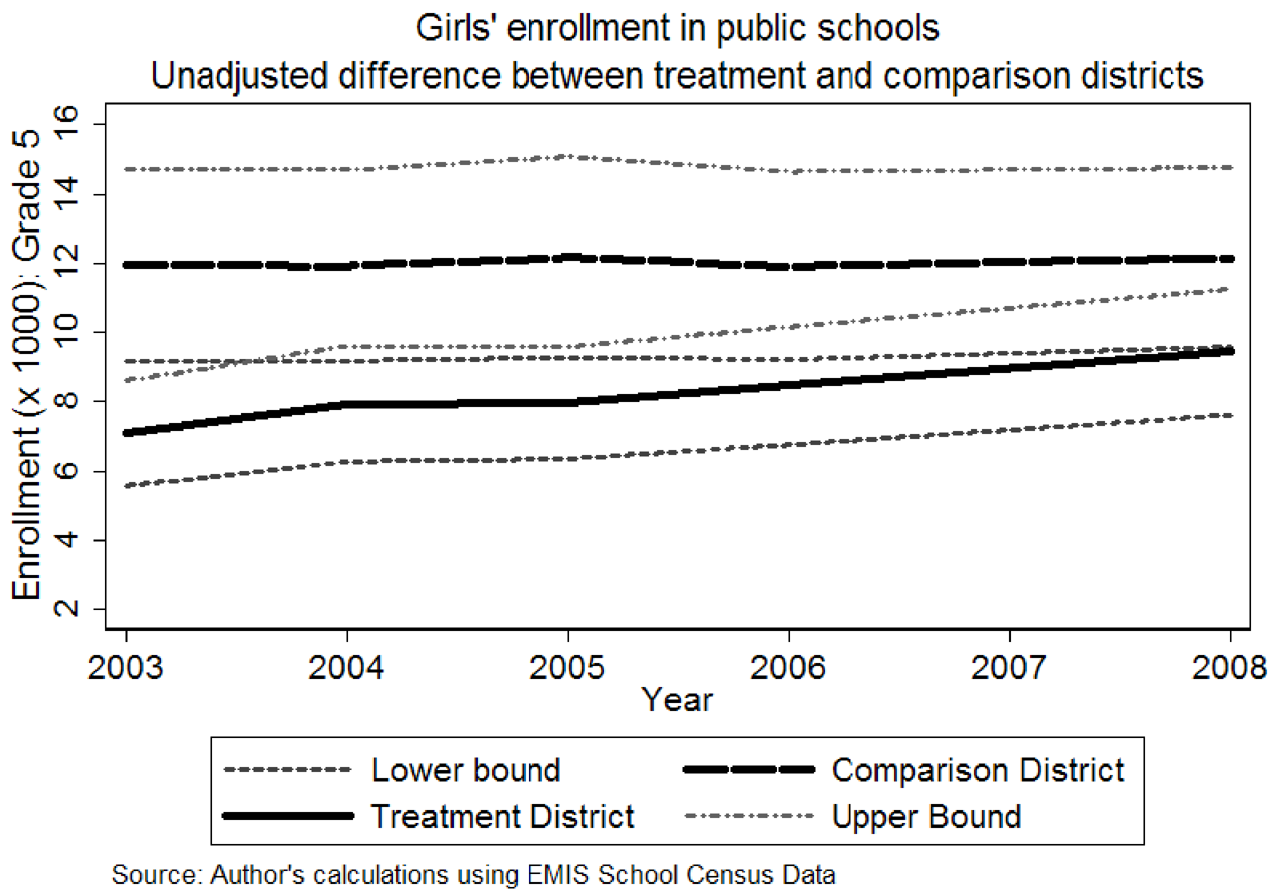
Note: Author's calculations using PMIU-PESRP program documentation. District map of Punjab province. Districts with a darker shade were treated. These were districts where less than 40 percent of the population aged 10 and over was estimated to be literate at the time of the 1998 Census. Districts with a lighter share were untreated. These were districts where more than 40 percent of the population aged 10 and over was literate.

Figure 3: Stylized description of methodology



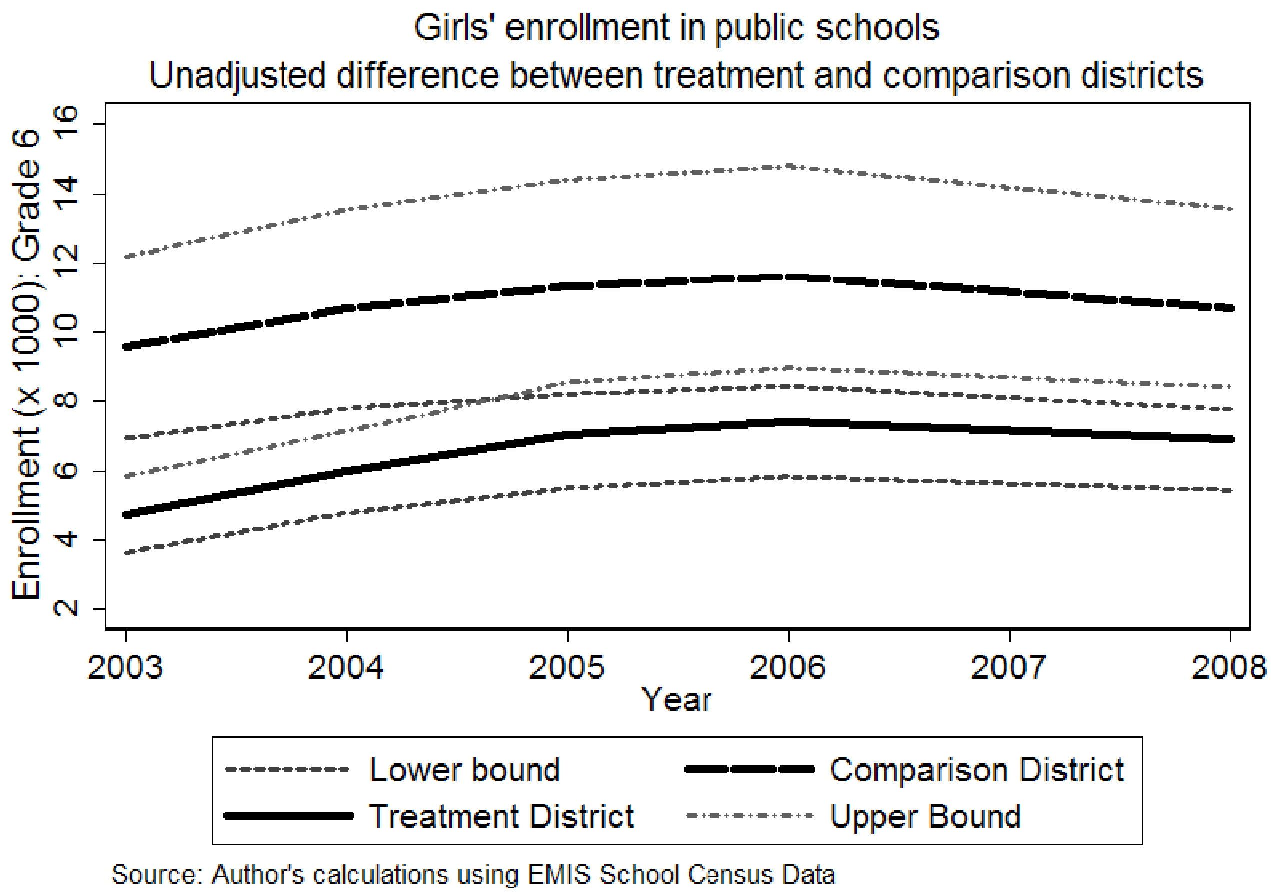
Vertical dashed line at time t represents the implementation of the program. Consequently t-1 is the baseline period and t+1 is the post-program period. Outcomes for two groups are shown: the comparison group (solid grey line) and the treated group (solid black line). The dotted line CD represents no change in the outcome of interest over time. The solid grey line AB represents the evolution of the outcome of the comparison group over time. Consequently, the distance BD is the natural trend in the outcome over time that would exist in the absence of any program. The line CEF shows how the outcome for the treated group would have evolved in the absence of the program. The difference between the two groups at baseline (the distance AC) would have persisted (the distance BF is drawn to equal the distance AC). However, the implementation of policy causes the outcome for the treated group to evolve differently. Consequently, the outcome for the treated group follows the path GH after the program is implemented. Using the difference-in-differences methodology allows the decomposition of outcomes over time into the a part due to a natural trend over time (BD), a part due to pre-existing differences between the two groups (BF) and the part that is due to the program (HF).

Figure 4: Girls' enrollment in Grade 5



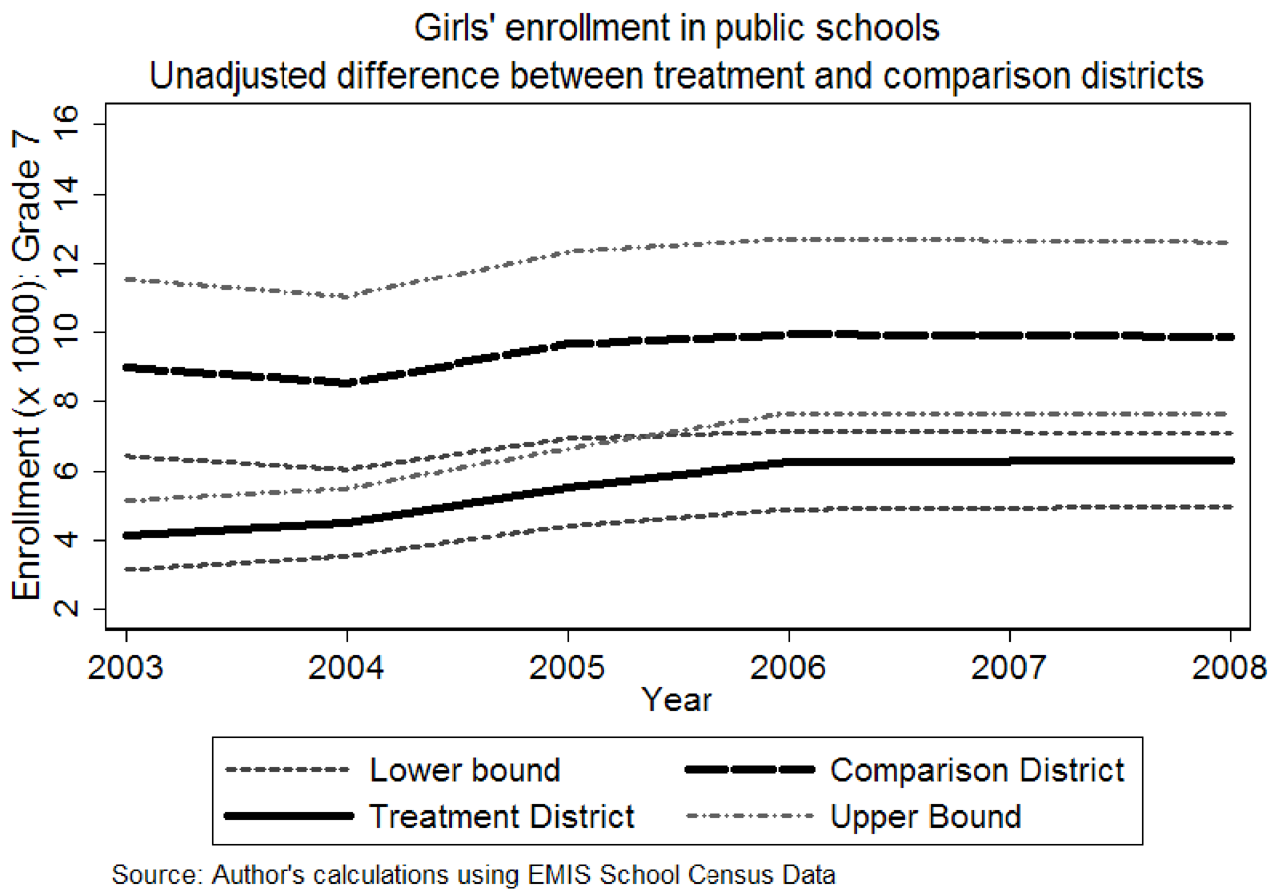
Note: Figures report average district enrollments over time. Solid lines represent treatment districts. Dashed lines represent control districts. Dotted lines and dashed lines with dots represent lower and upper bounds of the 95 percent confidence interval respectively.

Figure 5: Girls' enrollment in Grade 6



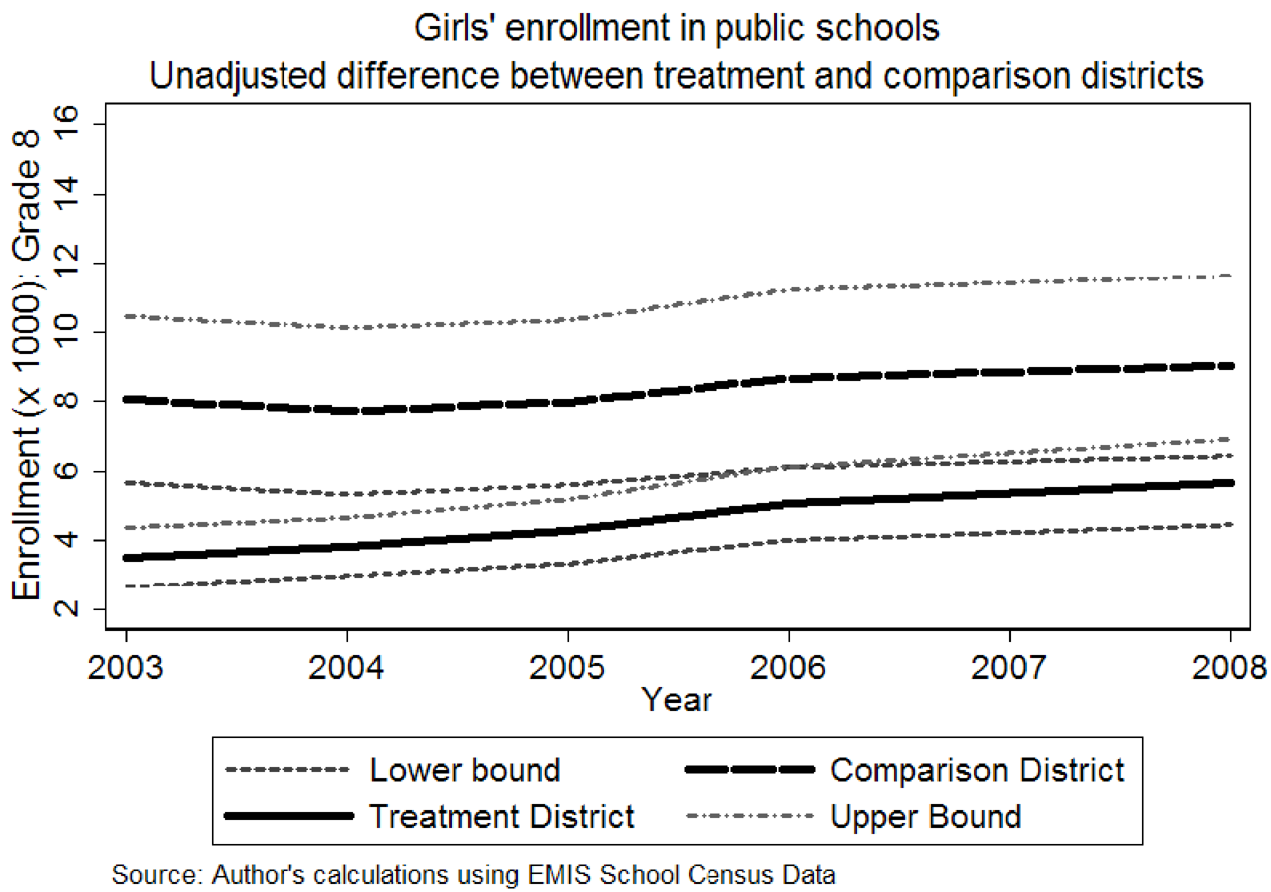
Note: Figures report average district enrollments over time. Solid lines represent treatment districts. Dashed lines represent control districts. Dotted lines and dashed lines with dots represent lower and upper bounds of the 95 percent confidence interval respectively.

Figure 6: Girls' enrollment in Grade 7



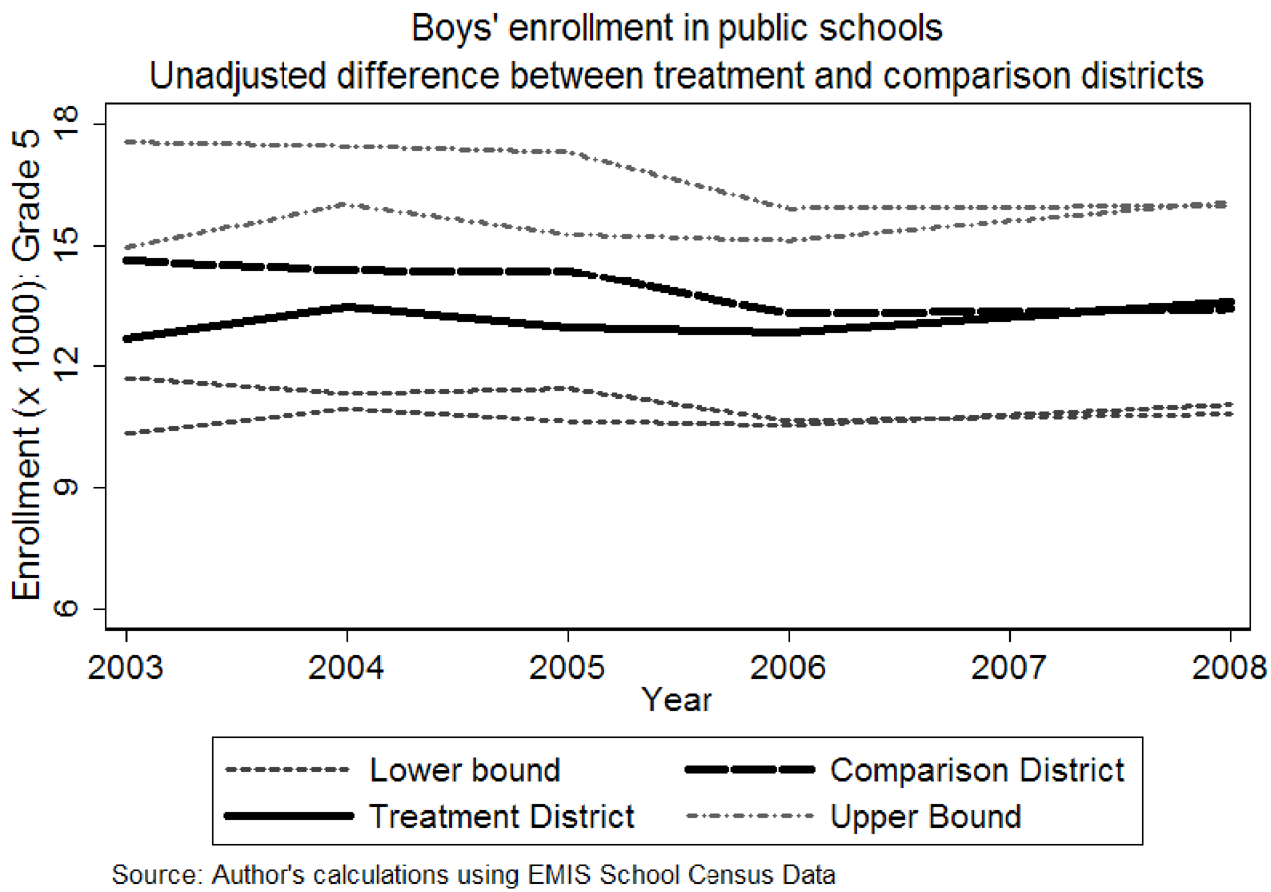
Note: Figures report average district enrollments over time. Solid lines represent treatment districts. Dashed lines represent control districts. Dotted lines and dashed lines with dots represent lower and upper bounds of the 95 percent confidence interval respectively.

Figure 7: Girls' enrollment in Grade 8



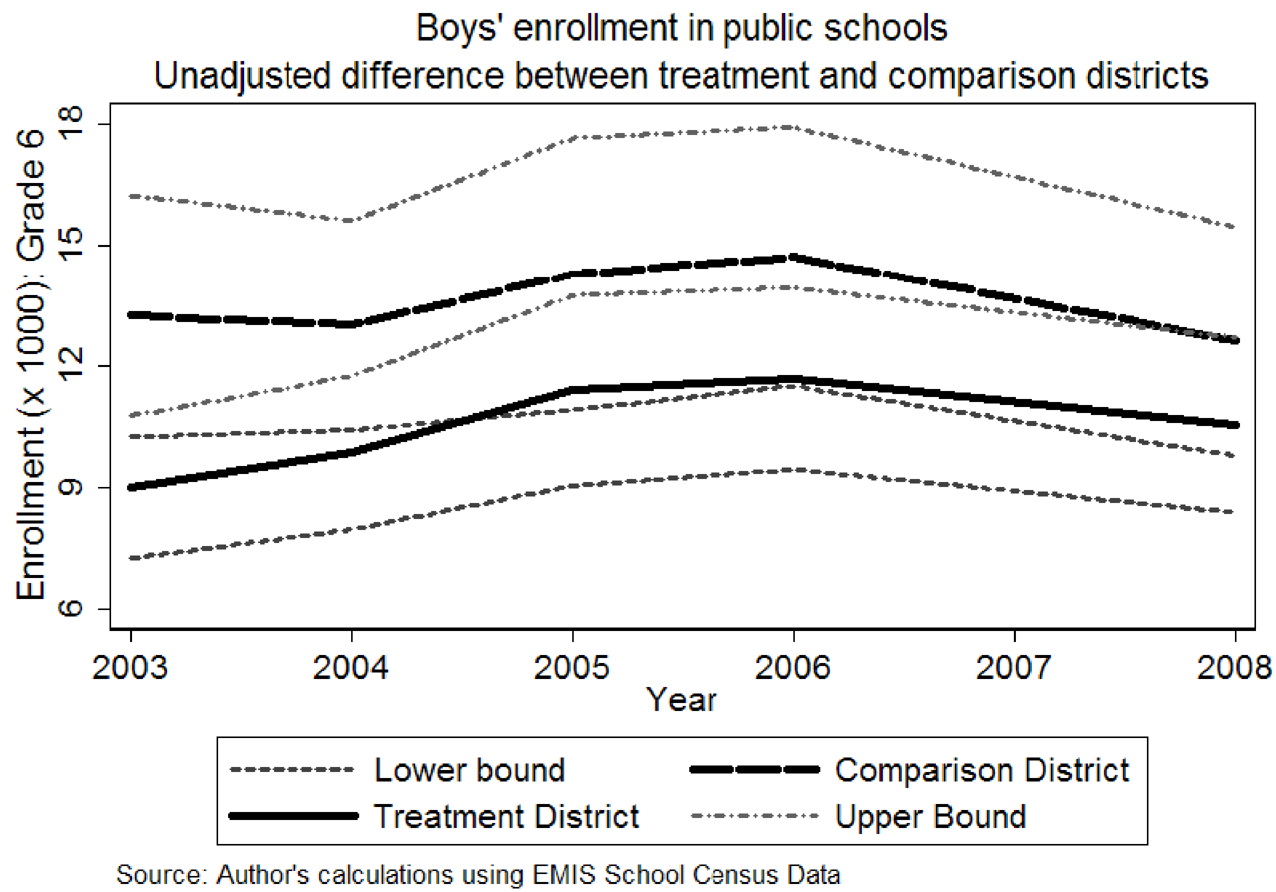
Note: Figures report average district enrollments over time. Solid lines represent treatment districts. Dashed lines represent control districts. Dotted lines and dashed lines with dots represent lower and upper bounds of the 95 percent confidence interval respectively.

Figure 8: Boys' enrollment in Grade 5



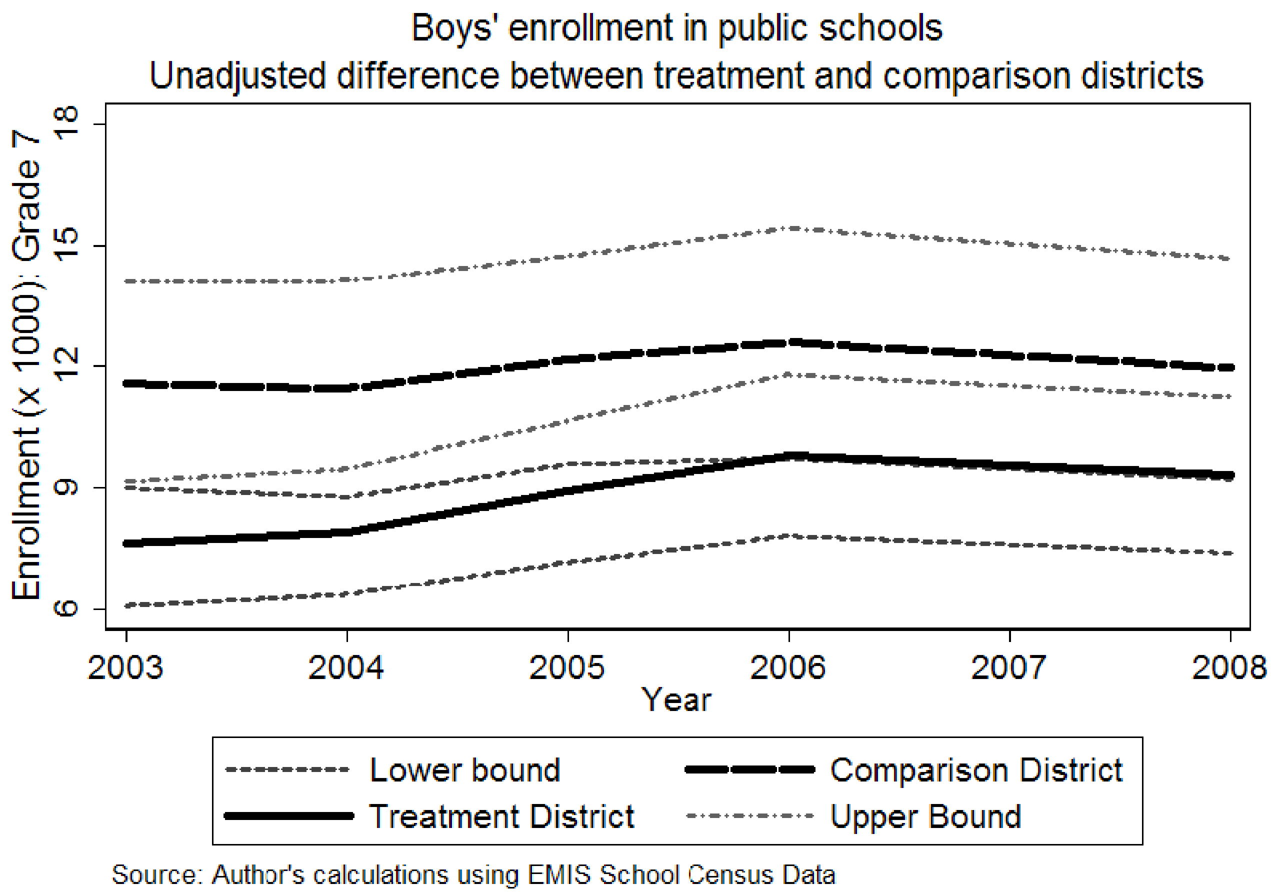
Note: Figures report average district enrollments over time. Solid lines represent treatment districts. Dashed lines represent control districts. Dotted lines and dashed lines with dots represent lower and upper bounds of the 95 percent confidence interval respectively.

Figure 9: Boys' enrollment in Grade 6



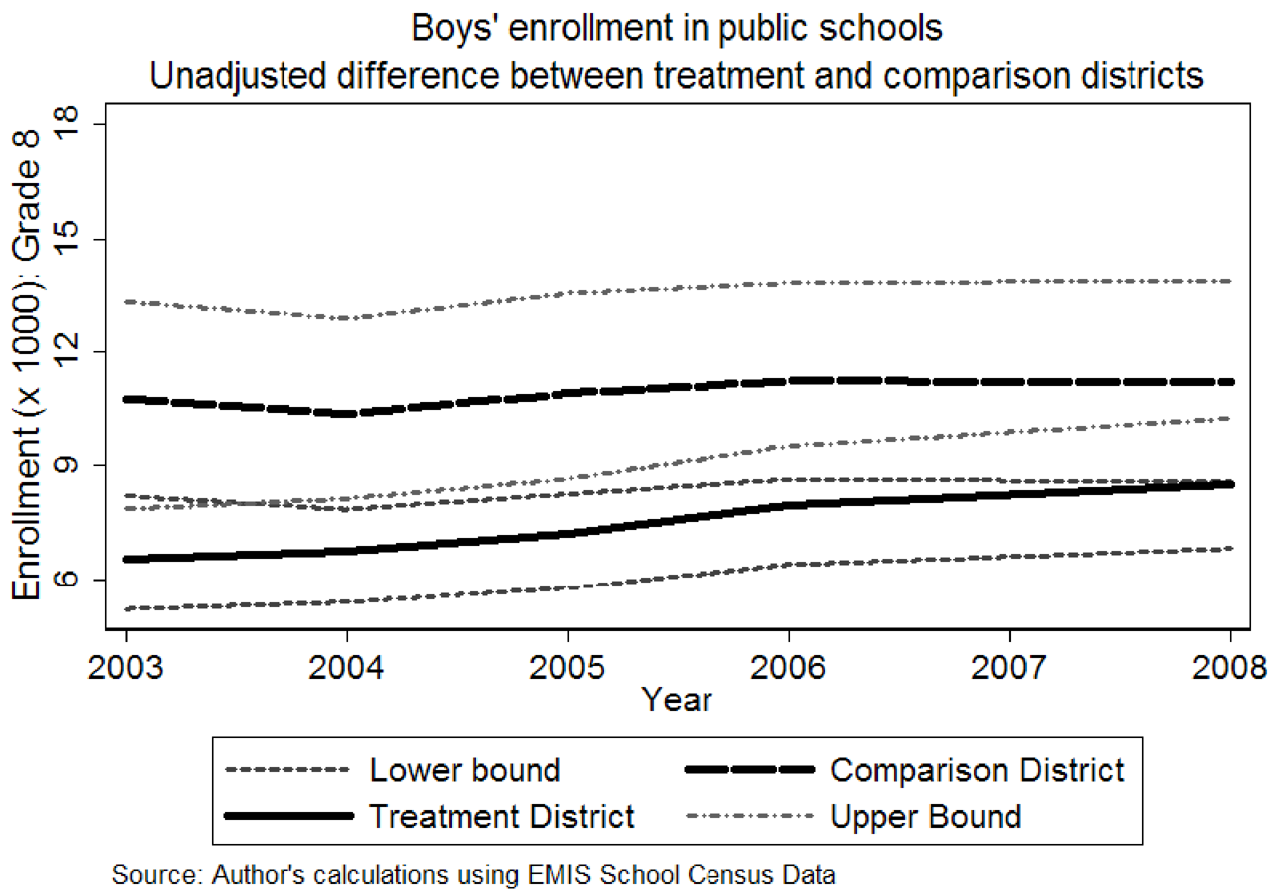
Note: Figures report average district enrollments over time. Solid lines represent treatment districts. Dashed lines represent control districts. Dotted lines and dashed lines with dots represent lower and upper bounds of the 95 percent confidence interval respectively.

Figure 10: Boys' enrollment in Grade 7



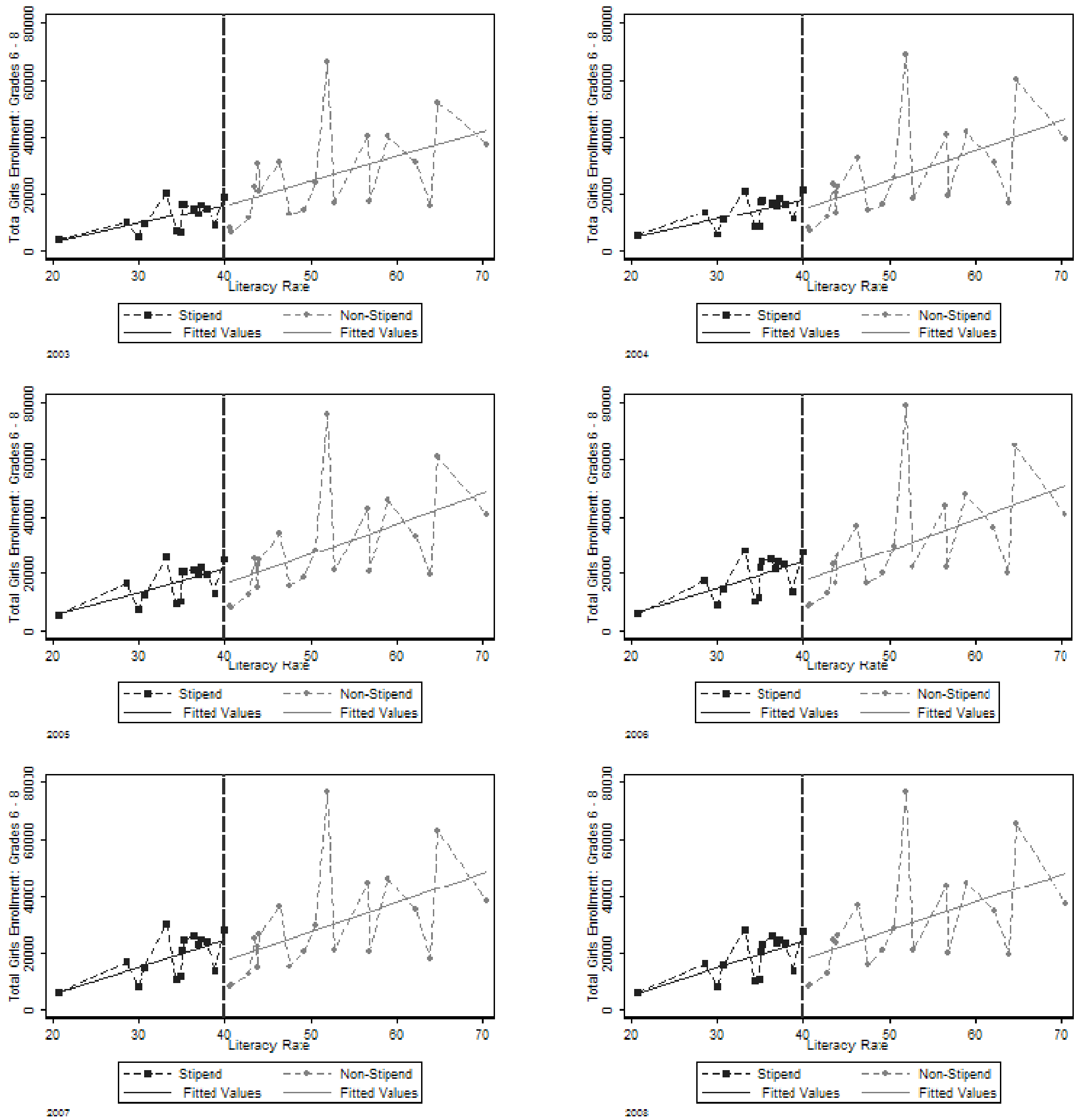
Note: Figures report average district enrollments over time. Solid lines represent treatment districts. Dashed lines represent control districts. Dotted lines and dashed lines with dots represent lower and upper bounds of the 95 percent confidence interval respectively.

Figure 11: Boys' enrollment in Grade 8



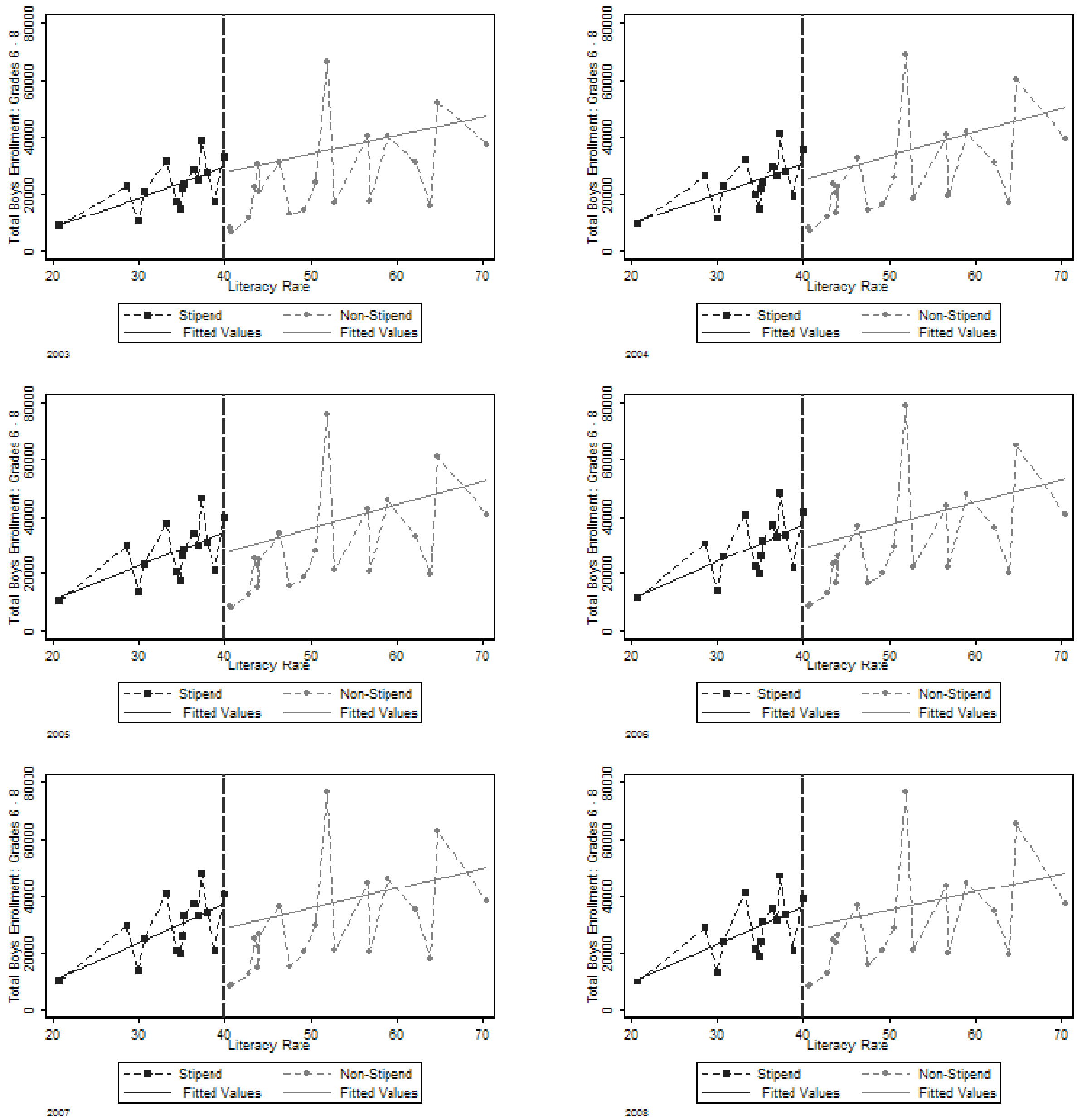
Note: Figures report average district enrollments over time. Solid lines represent treatment districts. Dashed lines represent control districts. Dotted lines and dashed lines with dots represent lower and upper bounds of the 95 percent confidence interval respectively.

Figure 12: Girls' enrollment around the literacy cutoff (District Averages)



Note: Average enrollment in grades 6 to 8 in treated and control districts reported. Vertical dashed line represents the literacy cutoff (40 percent) used to determine program eligibility. The black squares are treated districts. The grey circles are control districts. The first panel is the baseline year 2003. Each panel is a subsequent year. In 2003, there is no discontinuity at the cutoff as is to be expected. Over time a discontinuity emerges suggesting that the program is effective at increasing girls' enrollment in grades 6 to 8 taken together. The solid black and grey lines overlay the fitted values from a linear regression.

Figure 13: Boys' enrollment around the literacy cutoff (District Averages)



Note: Average enrollment in grades 6 to 8 in treated and control districts reported. Vertical dashed line represents the literacy cutoff (40 percent) used to determine program eligibility. The black squares are treated districts. The grey circles are control districts. The first panel is the baseline year 2003. Each panel is a subsequent year. In 2003, there is no discontinuity at the cutoff as is to be expected. Over time a discontinuity emerges suggesting that the program is effective at increasing boys' enrollment in grades 6 to 8 taken together. The solid black and grey lines overlay the fitted values from a linear regression.

Table 1: Baseline difference between treatment and control districts

	(1) Control	(2) Treatment	(3) Difference	(4) p-value
Number of Girls Class 5 (District total)	11,953	7,082	4,871	0.0051
Number of Girls Class 6 (District total)	9,566	4,750	4,816	0.0028
Number of Girls Class 7 (District total)	8,959	4,152	4,807	0.0020
Number of Girls Class 8 (District total)	8,051	3,507	4,543	0.0018
Number of Girls Class 9 (District total)	6,487	2,516	3,971	0.0019
Number of Girls Class 10 (District total)	5,211	2,021	3,189	0.0020
Number of Boys Class 5 (District total)	14,641	12,670	1,971	0.2920
Number of Boys Class 6 (District total)	13,256	9,031	4,225	0.0226
Number of Boys Class 7 (District total)	11,564	7,621	3,943	0.0138
Number of Boys Class 8 (District total)	10,759	6,540	4,219	0.0074
Number of Boys Class 9 (District total)	9,618	5,478	4,140	0.0043
Number of Boys Class 10 (District total)	6,844	3,901	2,943	0.0067
Drinking Water (1 = Yes)	0.806	0.810	-0.004	0.9124
Electricity (1 = Yes)	0.473	0.314	0.159	0.0012
Toilets (1 = Yes)	0.491	0.482	0.009	0.7502
Boundary Wall (1 = Yes)	0.611	0.514	0.097	0.0008
Main Gate (1 = Yes)	0.675	0.577	0.098	0.0021
Play Ground (1 = Yes)	0.354	0.379	-0.025	0.3404
Library (1 = Yes)	0.167	0.187	-0.020	0.3727
Physics Lab (1 = Yes)	0.266	0.252	0.014	0.6320
Biology Lab (1 = Yes)	0.191	0.166	0.025	0.3447
Chemistry Lab (1 = Yes)	0.210	0.184	0.026	0.3566
Home Economics Lab (1 = Yes)	0.038	0.034	0.003	0.7323
Computer Lab Morning (1 = Yes)	0.046	0.036	0.010	0.1997
Computer Lab Evening (1 = Yes)	0.019	0.009	0.010	0.0440
Observations	19	15		

Notes: Treatment districts have literacy rates < 40 percent. Control districts have literacy rates > 40 percent. Column 3 is calculated by subtracting column 2 from column 1. Enrollment figures are district totals for each grade and gender. The indicator variables describing school infrastructure and facilities are district averages representing the fraction of all public schools with the facility/infrastructure available. For instance, 49 percent of all schools in all control districts have toilets and 48 percent of all schools in all treatment districts have toilets. In contrast 61 percent of control district schools have a boundary wall while only 51 percent of treatment district schools do.

Table 2: Baseline difference between schools in treatment and control districts

	(1) Control	(2) Treatment	(3) Difference	(4) p-value
Number of Girls Class 5	7	4	3	0.0000
Number of Girls Class 6	26	16	10	0.0000
Number of Girls Class 7	24	14	10	0.0000
Number of Girls Class 8	21	12	10	0.0000
Number of Boys Class 5	9	7	2	0.0000
Number of Boys Class 6	36	30	6	0.0000
Number of Boys Class 7	31	25	6	0.0000
Number of Boys Class 8	29	22	7	0.0000
Ratio of Filled to Sanctioned Teaching Posts	0.850	0.868	-0.018	0.0000
Student-Teacher Ratio (All Classes/Sanctioned)	27.670	28.667	-0.997	0.0000
Student-Teacher Ratio (All Classes/Filled)	33.848	34.158	-0.310	0.0591
Rural (1 = Yes)	0.878	0.912	-0.034	0.0000
Toilets	0.492	0.480	0.011	0.0070
Drinking Water	0.819	0.811	0.008	0.0106
Electricity	0.466	0.304	0.162	0.0000
Boundary Wall	0.607	0.519	0.088	0.0000
Main Gate	0.678	0.581	0.097	0.0000
Play Ground	0.365	0.385	-0.020	0.0000
Library	0.178	0.207	-0.029	0.0000
Biology Lab	0.190	0.139	0.051	0.0000
Chemistry Lab	0.205	0.155	0.050	0.0000
Physics Lab	0.270	0.225	0.045	0.0000
Home Economics Lab	0.033	0.027	0.006	0.1278
Computer Lab (Morning)	0.042	0.030	0.012	0.0037
Computer Lab (Evening)	0.016	0.007	0.009	0.0003
Observations	30,283	26,620		

Notes: Treatment districts have literacy rates < 40 percent. Control districts have literacy rates > 40 percent. Column 3 is calculated by subtracting column 2 from column 1. Enrollment figures are school level averages. The indicator variables describing school infrastructure and facilities are representing the fraction of all public schools with the facility/infrastructure available. For instance, 49 percent of all schools in all control districts have toilets and 48 percent of all schools in all treatment districts have toilets. In contrast 61 percent of control district schools have a boundary wall while only 52 percent of treatment district schools do.

Table 3: Enrollment in Grades 6, 7 and 8

	School Fixed Effects Regression	
	Girls	**Boys**
	(1)	**(2)**
Stipend District		
2005	7.398***	10.524***
	(1.200)	(1.472)
Stipend District *2005	11.206***	9.777***
	(1.810)	(2.047)
Constant	93.221***	162.161***
	(0.459)	(0.540)
Number of observations	9,250	9,250
Number of schools	4,667	4,667
R2 within	0.041	0.040

Note: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable in Column 1 is total girls' enrollment in grades 6, 7 and 8. The dependent variable in Column 2 is total boys' enrollment in grades 6, 7 and 8. Schools in this sample are restricted to High Schools and Higher Secondary Schools. Only school census data for 2003 and 2005 are used in this analysis. Point estimates are similar to those reported in Chaudhury and Parajuli (2006), Table 4. Standard errors are robust to heteroskedasticity and clustered at the school level.

Table 4: Girls' Enrollment - Grade 5

	(1)	(2)	(3)	(4)	(5)
	OLS	FE	FE + Controls	Excluding mosque and primary schools	Excluding mosque and primary schools+Multan +Khanewal
Stipend District	-3.185***				
	(0.105)				
2004	0.156***	0.242***	0.377***	0.739**	0.756**
	(0.0397)	(0.0394)	(0.0417)	(0.273)	(0.289)
2005	0.338***	0.411***	0.545***	1.460***	1.494***
	(0.0433)	(0.0427)	(0.0439)	(0.281)	(0.297)
2006	0.364***	0.206***	0.210***	0.0838	0.0873
	(0.0460)	(0.0447)	(0.0452)	(0.172)	(0.171)
2007	-0.148***	-0.0567	-0.0459	-0.757***	-0.753***
	(0.0440)	(0.0436)	(0.0448)	(0.171)	(0.173)
2008	0.231***	0.320***	0.332***	-0.0865	-0.0729
	(0.0440)	(0.0436)	(0.0451)	(0.169)	(0.174)
Stipend District * 2004	0.175***	0.163**	0.165***	0.252	0.356
	(0.0501)	(0.0498)	(0.0496)	(0.199)	(0.209)
Stipend District * 2005	0.0425	0.0596	0.0618	0.0304	0.131
	(0.0543)	(0.0531)	(0.0527)	(0.216)	(0.226)
Stipend District * 2006	0.319***	0.467***	0.464***	1.011***	1.055***
	(0.0592)	(0.0581)	(0.0581)	(0.244)	(0.252)
Stipend District * 2007	0.835***	0.820***	0.820***	1.781***	1.831***
	(0.0561)	(0.0557)	(0.0558)	(0.224)	(0.233)
Stipend District * 2008	0.839***	0.826***	0.824***	1.616***	1.640***
	(0.0580)	(0.0577)	(0.0577)	(0.233)	(0.246)

Table 4: Girls' Enrollment - Grade 5

	(1)	(2)	(3)	(4)	(5)
	OLS	FE	FE + Controls	Excluding mosque and primary schools	Excluding mosque and primary schools+Multan +Khanewal
Number of Basic Facilities = 1			0.148***	0.641*	0.728*
			(0.0244)	(0.266)	(0.285)
Number of Basic Facilities = 2			0.197***	0.702**	0.753**
			(0.0271)	(0.263)	(0.281)
Number of Basic Facilities = 3			0.0961**	0.699**	0.752**
			(0.0368)	(0.267)	(0.286)
Constant	7.081***	5.544***	5.408***	12.42***	12.26***
	(0.0848)	(0.0189)	(0.0264)	(0.255)	(0.273)
Observations	345,196	345,196	345,196	69,047	64,476
Number of Schools		58,154	58,154	12,170	11,381
Adjusted R-squared	0.012	0.004	0.004	0.004	0.004
R-square within		0.00433	0.00442	0.00463	0.00465

* p<0.05 ** p<0.01 *** p<0.001. Standard errors in parentheses are robust to heteroskedasticity and clustered at the level of the school. The dependent variable is girls' enrollment in grade 5. The average school in a treatment district school at baseline had 4 girls enrolled in grade 5. Column 1 reports estimates from an OLS specification. Column 2 reports estimates from a fixed effects specification at the school level. Column 3 adds to column 2 a series of indicators for how many of the following three facilities a school has: toilets, drinking water and electricity. The excluded group is no facilities. The excluded year is the baseline year 2003. Column 4 excludes mosque schools and primary schools from the sample. Column 5 removes two districts whose treatment status was interchanged once the program began: Khanewal (changed from treatment to control) and Multan (changed from control to treatment).

Table 5: Girls' Enrollment - Grade 6

	(1)	(2)	(3)	(4)	(5)
	OLS	FE	FE + Controls	Excluding mosque and primary schools	Excluding mosque and primary schools+ Multan +Khanewal
Stipend District	-10.11***				
	(0.865)				
2004	3.234***	3.896***	2.482***	2.480***	2.609***
	(0.203)	(0.206)	(0.519)	(0.520)	(0.553)
2005	4.376***	5.375***	3.962***	3.967***	4.075***
	(0.251)	(0.253)	(0.536)	(0.537)	(0.570)
2006	1.790***	5.856***	6.182***	6.188***	6.368***
	(0.278)	(0.274)	(0.283)	(0.283)	(0.282)
2007	2.421***	3.789***	4.156***	4.154***	4.218***
	(0.265)	(0.261)	(0.272)	(0.272)	(0.281)
2008	1.469***	2.921***	3.317***	3.314***	3.414***
	(0.237)	(0.227)	(0.240)	(0.240)	(0.248)
Stipend District * 2004	0.263	0.0728	0.124	0.124	-0.0400
	(0.294)	(0.302)	(0.302)	(0.302)	(0.313)
Stipend District * 2005	1.863***	1.877***	1.932***	1.929***	1.854***
	(0.364)	(0.380)	(0.379)	(0.379)	(0.395)
Stipend District * 2006	2.379***	2.074***	2.072***	2.079***	1.859***
	(0.382)	(0.419)	(0.419)	(0.419)	(0.431)
Stipend District * 2007	3.829***	3.657***	3.640***	3.641***	3.510***
	(0.375)	(0.394)	(0.394)	(0.394)	(0.411)
Stipend District * 2008	3.511***	3.447***	3.438***	3.437***	3.259***
	(0.335)	(0.346)	(0.346)	(0.346)	(0.361)
Number of Basic Facilities = 1			-0.326	-0.324	-0.288
			(0.510)	(0.512)	(0.546)
Number of Basic Facilities = 2			-0.954	-0.952	-0.860
			(0.502)	(0.504)	(0.537)

Table 5: Girls' Enrollment - Grade 6

	(1)	(2)	(3)	(4)	(5)
	OLS	FE	FE + Controls	Excluding mosque and primary schools	Excluding mosque and primary schools+ Multan +Khanewal
Number of Basic Facilities = 3			-1.975***	-1.977***	-1.888***
			(0.509)	(0.510)	(0.544)
Constant	25.74***	20.23***	21.60***	22.21***	22.14***
	(0.639)	(0.133)	(0.494)	(0.496)	(0.530)
Observations	71,620	71,620	71,620	70,074	65,476
Number of Schools		13,675	13,675	12,165	11,377
Adjusted R-squared	0.007	0.037	0.038	0.038	0.039
R-square within		0.0375	0.0383	0.0383	0.0389

* p<0.05 ** p<0.01 *** p<0.001. Standard errors in parentheses are robust to heteroskedasticity and clustered at the level of the school. The dependent variable is girls' enrollment in grade 6. The average school in a treatment district school at baseline had 16 girls enrolled in grade 6. Column 1 reports estimates from an OLS specification. Column 2 reports estimates from a fixed effects specification at the school level. Column 3 adds to column 2 a series of indicators for how many of the following three facilities a school has: toilets, drinking water and electricity. The excluded group is no facilities. The excluded year is the baseline year 2003. Column 4 excludes mosque schools and primary schools from the sample. Column 5 removes two districts whose treatment status was interchanged once the program began: Khanewal (changed from treatment to control) and Multan (changed from control to treatment).

Table 6: Girls' Enrollment - Grade 7

	(1)	(2)	(3)	(4)	(5)
	OLS	FE	FE + Controls	Excluding mosque and primary schools	Excluding mosque and primary schools + Multan + Khanewal
Stipend District	-10.30***				
	(0.833)				
2004	-0.999***	-0.369*	-0.418	-0.421	-0.526
	(0.177)	(0.168)	(0.464)	(0.465)	(0.486)
2005	1.572***	2.537***	2.488***	2.494***	2.447***
	(0.230)	(0.223)	(0.481)	(0.482)	(0.503)
2006	-0.575*	2.995***	3.053***	3.053***	3.202***
	(0.257)	(0.232)	(0.240)	(0.240)	(0.238)
2007	1.222***	2.582***	2.649***	2.648***	2.727***
	(0.253)	(0.242)	(0.250)	(0.250)	(0.259)
2008	1.022***	2.496***	2.568***	2.565***	2.614***
	(0.237)	(0.223)	(0.236)	(0.236)	(0.244)
Stipend District * 2004	1.858***	1.622***	1.631***	1.632***	1.751***
	(0.232)	(0.225)	(0.225)	(0.225)	(0.235)
Stipend District * 2005	2.063***	1.991***	2.001***	1.995***	1.940***
	(0.311)	(0.314)	(0.314)	(0.314)	(0.329)
Stipend District * 2006	3.759***	3.482***	3.483***	3.487***	3.391***
	(0.337)	(0.349)	(0.349)	(0.350)	(0.358)
Stipend District * 2007	4.596***	4.337***	4.335***	4.337***	4.286***
	(0.347)	(0.358)	(0.358)	(0.358)	(0.374)
Stipend District * 2008	4.092***	3.910***	3.909***	3.912***	3.932***
	(0.324)	(0.332)	(0.333)	(0.333)	(0.348)

Table 6: Girls' Enrollment - Grade 7

	(1)	(2)	(3)	(4)	(5)
	OLS	FE	FE + Controls	Excluding mosque and primary schools	Excluding mosque and primary schools + Multan + Khanewal
Number of Basic Facilities = 1			0.161	0.160	0.0689
			(0.449)	(0.450)	(0.472)
Number of Basic Facilities = 2			0.0411	0.0393	-0.0269
			(0.449)	(0.451)	(0.472)
Number of Basic Facilities = 3			-0.151	-0.153	-0.252
			(0.456)	(0.457)	(0.479)
Constant	23.97***	18.50***	18.54***	19.05***	19.17***
	(0.627)	(0.114)	(0.444)	(0.447)	(0.468)
Observations	71,607	71,607	71,607	70,065	65,469
Number of Schools		13,674	13,674	12,165	11,377
Adjusted R-squared	0.007	0.036	0.036	0.036	0.037
R-square within		0.0362	0.0362	0.0362	0.0371

* p<0.05 ** p<0.01 *** p<0.001. Standard errors in parentheses are robust to heteroskedasticity and clustered at the level of the school. The dependent variable is girls' enrollment in grade 7. The average school in a treatment district school at baseline had 14 girls enrolled in grade 7. Column 1 reports estimates from an OLS specification. Column 2 reports estimates from a fixed effects specification at the school level. Column 3 adds to column 2 a series of indicators for how many of the following three facilities a school has: toilets, drinking water and electricity. The excluded group is no facilities. The excluded year is the baseline year 2003. Column 4 excludes mosque schools and primary schools from the sample. Column 5 removes two districts whose treatment status was interchanged once the program began: Khanewal (changed from treatment to control) and Multan (changed from control to treatment).

Table 7: Girls' Enrollment - Grade 8

	(1)	(2)	(3)	(4)	(5)
	OLS	FE	FE + Controls	Excluding mosque and primary schools	Excluding mosque and primary schools + Multan + Khanewal
Stipend District	-9.749***				
	(0.789)				
2004	-0.676***	-0.0513	0.462	0.461	0.469
	(0.196)	(0.191)	(0.566)	(0.567)	(0.606)
2005	-0.283	0.675**	1.188*	1.191*	1.196
	(0.240)	(0.230)	(0.589)	(0.590)	(0.629)
2006	-1.030***	2.186***	2.144***	2.143***	2.304***
	(0.268)	(0.251)	(0.263)	(0.263)	(0.263)
2007	0.807**	2.200***	2.153***	2.153***	2.153***
	(0.260)	(0.251)	(0.265)	(0.266)	(0.275)
2008	1.591***	3.102***	3.051***	3.050***	3.103***
	(0.250)	(0.242)	(0.258)	(0.258)	(0.269)
Stipend District * 2004	1.371***	1.108***	1.103***	1.103***	1.130***
	(0.237)	(0.232)	(0.231)	(0.231)	(0.243)
Stipend District * 2005	2.051***	1.919***	1.913***	1.910***	2.021***
	(0.289)	(0.283)	(0.282)	(0.282)	(0.297)
Stipend District * 2006	3.119***	2.678***	2.680***	2.681***	2.498***
	(0.338)	(0.346)	(0.346)	(0.346)	(0.352)
Stipend District * 2007	4.403***	4.055***	4.059***	4.061***	4.121***
	(0.337)	(0.346)	(0.347)	(0.347)	(0.359)
Stipend District * 2008	3.787***	3.501***	3.505***	3.507***	3.529***
	(0.332)	(0.343)	(0.343)	(0.343)	(0.358)

Table 7: Girls' Enrollment - Grade 8

	(1)	(2)	(3)	(4)	(5)
	OLS	FE	FE + Controls	Excluding mosque and primary schools	Excluding mosque and primary schools + Multan + Khanewal
Number of Basic Facilities = 1			0.413	0.412	0.452
			(0.542)	(0.543)	(0.582)
Number of Basic Facilities = 2			0.460	0.459	0.488
			(0.548)	(0.550)	(0.589)
Number of Basic Facilities = 3			0.585	0.584	0.573
			(0.557)	(0.559)	(0.600)
Constant	21.29***	16.13***	15.63***	16.05***	16.09***
	(0.605)	(0.118)	(0.546)	(0.549)	(0.588)
Observations	71600	71600	71600	70058	65463
Number of Schools		13674	13674	12165	11377
Adjusted R-squared	0.007	0.032	0.032	0.032	0.032
R-square within		0.0320	0.0320	0.0320	0.0327

* p<0.05 ** p<0.01 *** p<0.001. Standard errors in parentheses are robust to heteroskedasticity and clustered at the level of the school. The dependent variable is girls' enrollment in grade 8. The average school in a treatment district school at baseline had 12 girls enrolled in grade 8. Column 1 reports estimates from an OLS specification. Column 2 reports estimates from a fixed effects specification at the school level. Column 3 adds to column 2 a series of indicators for how many of the following three facilities a school has: toilets, drinking water and electricity. The excluded group is no facilities. The excluded year is the baseline year 2003. Column 4 excludes mosque schools and primary schools from the sample. Column 5 removes two districts whose treatment status was interchanged once the program began: Khanewal (changed from treatment to control) and Multan (changed from control to treatment).

Table 8: Boys' Enrollment - Grades 5 to 8

	(1)	(2)	(3)	(4)
	Class 5	Class 6	Class 7	Class 8
Stipend District				
2004	0.246	0.333	1.143*	0.626
	(0.268)	(0.484)	(0.449)	(0.455)
2005	0.445	3.940***	3.030***	2.051***
	(0.274)	(0.505)	(0.468)	(0.470)
2006	-1.426***	5.126***	4.029***	2.354***
	(0.140)	(0.330)	(0.287)	(0.276)
2007	-1.547***	1.895***	3.944***	1.723***
	(0.137)	(0.304)	(0.281)	(0.270)
2008	-1.131***	-0.811**	2.139***	2.295***
	(0.138)	(0.301)	(0.259)	(0.255)
Stipend District * 2004	0.543**	1.847***	-0.187	0.563*
	(0.184)	(0.355)	(0.290)	(0.270)
Stipend District * 2005	0.0652	3.070***	1.144**	0.620*
	(0.194)	(0.411)	(0.348)	(0.299)
Stipend District * 2006	1.071***	2.705***	2.549***	2.009***
	(0.201)	(0.462)	(0.407)	(0.363)
Stipend District * 2007	1.218***	3.615***	2.329***	3.921***
	(0.197)	(0.413)	(0.388)	(0.358)
Stipend District * 2008	1.675***	4.926***	2.836***	3.686***
	(0.202)	(0.409)	(0.367)	(0.348)
Constant	15.16***	32.48***	27.30***	24.14***
	(0.250)	(0.449)	(0.424)	(0.432)
Observations	69047	70074	70065	70058
Number of Schools	12170	12165	12165	12165
Adjusted R-squared	0.011	0.029	0.026	0.020
R-square within	0.0108	0.0291	0.0265	0.0197
Controls Included	Y	Y	Y	Y

* p<0.05 ** p<0.01 *** p<0.001. Standard errors in parentheses are robust and clustered at the school level. School-level fixed effects regressions reported. Dependent variable is boys' enrollment in grades 5 to 8. Controls include a series of indicators for how many of the following three facilities a school has: toilets, drinking water and electricity. The excluded group is no facilities. The excluded year is 2003. Each column excludes mosque and primary schools and two districts whose treatment status was interchanged once the program began: Khanewal and Multan. The average school in a treatment district at baseline has 7, 30, 25 and 22 students in grades 5, 6, 7 and 8 respectively.

Table 9: Ratio of All Students to Filled Teaching Positions

	(1)	(2)	(3)	(4)
	OLS	FE	FE + Controls	Excluding Multan + Khanewal
Stipend District	-4.111***			
	(0.272)			
2004	1.598***	1.413***	3.158***	3.077***
	(0.156)	(0.149)	(0.823)	(0.878)
2005	2.482***	2.233***	3.979***	3.877***
	(0.175)	(0.168)	(0.832)	(0.887)
2006	4.167***	3.856***	3.825***	3.665***
	(0.219)	(0.212)	(0.216)	(0.223)
2007	1.399***	1.004***	0.982***	0.670***
	(0.184)	(0.176)	(0.183)	(0.190)
2008	1.162***	0.658***	0.629**	0.361
	(0.197)	(0.186)	(0.195)	(0.202)
Stipend District * 2004	1.697***	1.728***	1.714***	1.941***
	(0.218)	(0.207)	(0.207)	(0.220)
Stipend District * 2005	3.042***	2.914***	2.900***	3.033***
	(0.246)	(0.236)	(0.236)	(0.249)
Stipend District * 2006	4.556***	4.431***	4.429***	4.707***
	(0.315)	(0.304)	(0.304)	(0.322)
Stipend District * 2007	6.037***	5.681***	5.679***	6.109***
	(0.277)	(0.259)	(0.259)	(0.273)
Stipend District * 2008	5.934***	5.478***	5.474***	5.770***
	(0.299)	(0.276)	(0.276)	(0.292)
Number of Basic Facilities = 1			1.429	1.394
			(0.867)	(0.927)
Number of Basic Facilities = 2			1.865*	1.839*
			(0.835)	(0.892)
Number of Basic Facilities = 3			1.776*	1.785*
			(0.820)	(0.877)
Constant	31.15***	29.86***	28.12***	28.17***
	(0.193)	(0.0907)	(0.823)	(0.880)
Observations	70160	70160	70160	65562
Number of Schools		12168	12168	11380
Adjusted R-squared	0.017	0.049	0.049	0.048
R-square within		0.0494	0.0496	0.0483

* p<0.05 ** p<0.01 *** p<0.001. Standard errors in parentheses are robust to heteroskedasticity and clustered at the level of the school. The dependent variable is the ratio of students in a given school to filled teaching posts. The average school in a treatment district school at baseline had a ratio of 34. Sample excludes mosque and primary schools. Column 1 reports estimates from an OLS specification. Column 2 reports estimates from a fixed effects specification at the school level. Column 3 adds a series of indicators for how many of the following three facilities a school has: toilets, drinking water and electricity. The excluded group is no facilities. The excluded year is 2003. Column 4 removes two districts whose treatment status was interchanged: Khanewal and Multan.

Table 10: Ratio of All Students to Sanctioned Teaching Positions

	(1)	(2)	(3)	(4)
	OLS	FE	FE + Controls	Excluding Multan + Khanewal
Stipend District	-3.068***			
	(0.214)			
2004	3.438***	3.335***	6.050***	5.688***
	(0.143)	(0.137)	(0.688)	(0.703)
2005	5.797***	5.731***	8.444***	8.065***
	(0.152)	(0.148)	(0.697)	(0.712)
2006	3.246***	3.198***	2.866***	2.681***
	(0.162)	(0.154)	(0.163)	(0.167)
2007	0.858***	0.822***	0.460**	0.229
	(0.153)	(0.149)	(0.172)	(0.179)
2008	-0.367**	-0.430***	-0.825***	-1.007***
	(0.128)	(0.126)	(0.141)	(0.146)
Stipend District * 2004	2.201***	2.220***	2.156***	2.268***
	(0.212)	(0.205)	(0.204)	(0.215)
Stipend District * 2005	2.977***	2.990***	2.921***	2.915***
	(0.223)	(0.218)	(0.218)	(0.230)
Stipend District * 2006	4.250***	4.289***	4.283***	4.480***
	(0.232)	(0.226)	(0.226)	(0.238)
Stipend District * 2007	4.647***	4.678***	4.686***	4.988***
	(0.200)	(0.194)	(0.196)	(0.206)
Stipend District * 2008	4.653***	4.660***	4.659***	4.843***
	(0.181)	(0.177)	(0.177)	(0.186)
Number of Basic Facilities = 1			1.271	0.952
			(0.678)	(0.693)
Number of Basic Facilities = 2			2.432***	2.116**
			(0.693)	(0.709)
Number of Basic Facilities = 3			3.260***	2.925***
			(0.696)	(0.712)
Constant	24.27***	23.08***	20.40***	20.70***
	(0.151)	(0.0685)	(0.682)	(0.698)
Observations	70205	70205	70205	65601
Number of Schools		12170	12170	11381
Adjusted R-squared	0.035	0.103	0.105	0.103
R-square within		0.103	0.105	0.103

* p<0.05 ** p<0.01 *** p<0.001. Standard errors in parentheses are robust to heteroskedasticity and clustered at the level of the school. The dependent variable is the ratio of students in a given school to sanctioned teaching posts. The average school in a treatment district school at baseline had a ratio of 29. Sample excludes mosque and primary schools. Column 1 reports estimates from an OLS specification. Column 2 reports estimates from a fixed effects specification at the school level. Column 3 adds a series of indicators for how many of the following three facilities a school has: toilets, drinking water and electricity. The excluded group is no facilities. The excluded year is the baseline year 2003. Column 4 removes two districts whose treatment status was interchanged: Khanewal and Multan.

Table 11: Ratio of students to teaching positions by gender of school

	(1)	(2)	(3)	(4)
	Filled		Sanctioned	
School fixed effects regressions	Girls	Boys	Girls	Boys
2004	3.758***	-0.261	6.412***	0.0348
	(1.012)	(0.640)	(0.782)	(0.141)
2005	3.909***	0.225	7.893***	0.122
	(1.026)	(0.648)	(0.801)	(0.142)
2006	3.680***	2.220***	2.829***	0.0134
	(0.258)	(0.280)	(0.201)	(0.0531)
2007	0.894***	-0.326	0.577*	0.0517
	(0.238)	(0.204)	(0.259)	(0.0575)
2008	1.188***	-0.329	-0.0789	-0.00941
	(0.251)	(0.209)	(0.190)	(0.0553)
Stipend District * 2004	1.383***	1.432***	1.370***	0.0890
	(0.266)	(0.215)	(0.270)	(0.0526)
Stipend District * 2005	3.030***	2.169***	2.390***	0.0279
	(0.314)	(0.259)	(0.293)	(0.0548)
Stipend District * 2006	4.421***	2.537***	3.945***	0.0981
	(0.398)	(0.345)	(0.314)	(0.0543)
Stipend District * 2007	6.521***	4.045***	4.886***	0.209**
	(0.355)	(0.277)	(0.287)	(0.0691)
Stipend District * 2008	6.912***	3.824***	5.121***	0.0577
	(0.389)	(0.281)	(0.240)	(0.0538)
Constant	22.70***	26.14***	16.29***	0.153
	(1.007)	(0.630)	(0.766)	(0.138)
Adjusted R-squared	0.063	0.033	0.094	0.002
R-square within	0.0636	0.0334	0.0943	0.00281
Observations	32,545	33,017	32,577	33,024
Number of Schools	5,718	5,683	5,719	5,683
Controls included	Yes	Yes	Yes	Yes
Multan and Khanewal Excluded	Yes	Yes	Yes	Yes

* p<0.05 ** p<0.01 *** p<0.001. Standard errors in parentheses are robust to heteroskedasticity and clustered at the level of the school. Sample excludes mosque and primary schools and two districts whose treatment status was interchanged. Estimates from a fixed effects specification at the school level. Controls are a series of indicators for how many of the following three facilities a school has: toilets, drinking water and electricity. The excluded group is no facilities. The excluded year is the baseline year 2003.

Table 12: Heterogeneous Treatment Effects - Subsample of rural schools

	(1)	(2)	(3)	(4)
	Grade 5	Grade 6	Grade 7	Grade 8
Stipend District				
2004	0.205	3.339***	-0.253	0.0325
	(0.107)	(0.176)	(0.134)	(0.126)
2005	0.906***	4.845***	2.398***	0.716***
	(0.122)	(0.214)	(0.159)	(0.144)
2006	0.363**	5.705***	3.357***	2.493***
	(0.131)	(0.224)	(0.172)	(0.164)
2007	-0.322*	4.028***	3.237***	2.805***
	(0.126)	(0.212)	(0.185)	(0.172)
2008	0.406**	3.300***	3.071***	3.688***
	(0.133)	(0.195)	(0.171)	(0.164)
Stipend District * 2004	0.00803	-0.553*	1.368***	0.776***
	(0.187)	(0.249)	(0.185)	(0.166)
Stipend District * 2005	-0.126	0.455	1.110***	1.482***
	(0.202)	(0.307)	(0.231)	(0.201)
Stipend District * 2006	0.528*	0.289	1.693***	1.321***
	(0.207)	(0.341)	(0.273)	(0.248)
Stipend District * 2007	1.441***	1.747***	2.384***	2.276***
	(0.212)	(0.331)	(0.282)	(0.257)
Stipend District * 2008	1.305***	1.839***	2.347***	1.908***
	(0.223)	(0.297)	(0.267)	(0.261)
Constant	10.65***	14.70***	12.82***	10.50***
	(0.0708)	(0.110)	(0.0866)	(0.0798)
Observations	54419	54982	54977	54970
Number of Schools	9590	9586	9586	9586
Adjusted R-squared	0.007	0.049	0.059	0.063
R-square within	0.00722	0.0494	0.0594	0.0632

* p<0.05 ** p<0.01 *** p<0.001. Standard errors in parentheses are robust to heteroskedasticity and clustered at the level of the school. The dependent variable in each column is girls' enrollment in a particular grade. The sample excludes mosque and primary schools as well as two districts whose treatment status was interchanged once the program began: Khanewal (changed from treatment to control) and Multan (changed from control to treatment. All columns report estimates from a fixed effects specification at the school level. The excluded group is no facilities. The excluded year is the baseline year 2003.

Table 13: Heterogeneous Treatment Effects - Subsample of urban schools

	(1)	(2)	(3)	(4)
	Grade 5	Grade 6	Grade 7	Grade 8
Stipend District				
2004	-0.790	6.578***	-0.994	-0.400
	(0.604)	(0.857)	(0.736)	(0.926)
2005	0.128	7.892***	3.462**	0.545
	(0.701)	(1.066)	(1.052)	(1.136)
2006	-1.068	7.546***	2.163*	1.679
	(0.727)	(1.093)	(0.980)	(1.142)
2007	-2.616***	3.012**	0.0145	-0.582
	(0.672)	(1.123)	(1.083)	(1.185)
2008	-2.141**	1.704	0.116	0.695
	(0.659)	(0.926)	(1.001)	(1.155)
Stipend District * 2004	2.237**	4.196**	3.771***	3.167**
	(0.841)	(1.514)	(1.118)	(1.212)
Stipend District * 2005	1.478	11.87***	7.582***	5.316***
	(0.899)	(1.937)	(1.712)	(1.484)
Stipend District * 2006	4.268**	12.92***	14.08***	9.816***
	(1.353)	(2.117)	(1.784)	(1.813)
Stipend District * 2007	3.445***	14.85***	15.59***	14.95***
	(0.911)	(1.953)	(1.881)	(1.798)
Stipend District * 2008	2.773**	12.00***	13.16***	12.98***
	(0.996)	(1.708)	(1.725)	(1.778)
Constant	25.65***	53.10***	51.68***	48.68***
	(0.358)	(0.603)	(0.564)	(0.621)
Observations	10057	10494	10492	10493
Number of Schools	1791	1791	1791	1791
Adjusted R-squared	0.006	0.047	0.035	0.024
R-square within	0.00694	0.0480	0.0361	0.0246

* p<0.05 ** p<0.01 *** p<0.001. Standard errors in parentheses are robust to heteroskedasticity and clustered at the level of the school. The dependent variable in each column is girls' enrollment in a particular grade. The sample excludes mosque and primary schools as well as two districts whose treatment status was interchanged once the program began: Khanewal (changed from treatment to control) and Multan (changed from control to treatment. All columns report estimates from a fixed effects specification at the school level. The excluded group is no facilities. The excluded year is the baseline year 2003.

Figure 14: Fraction of girls' schools at the primary level

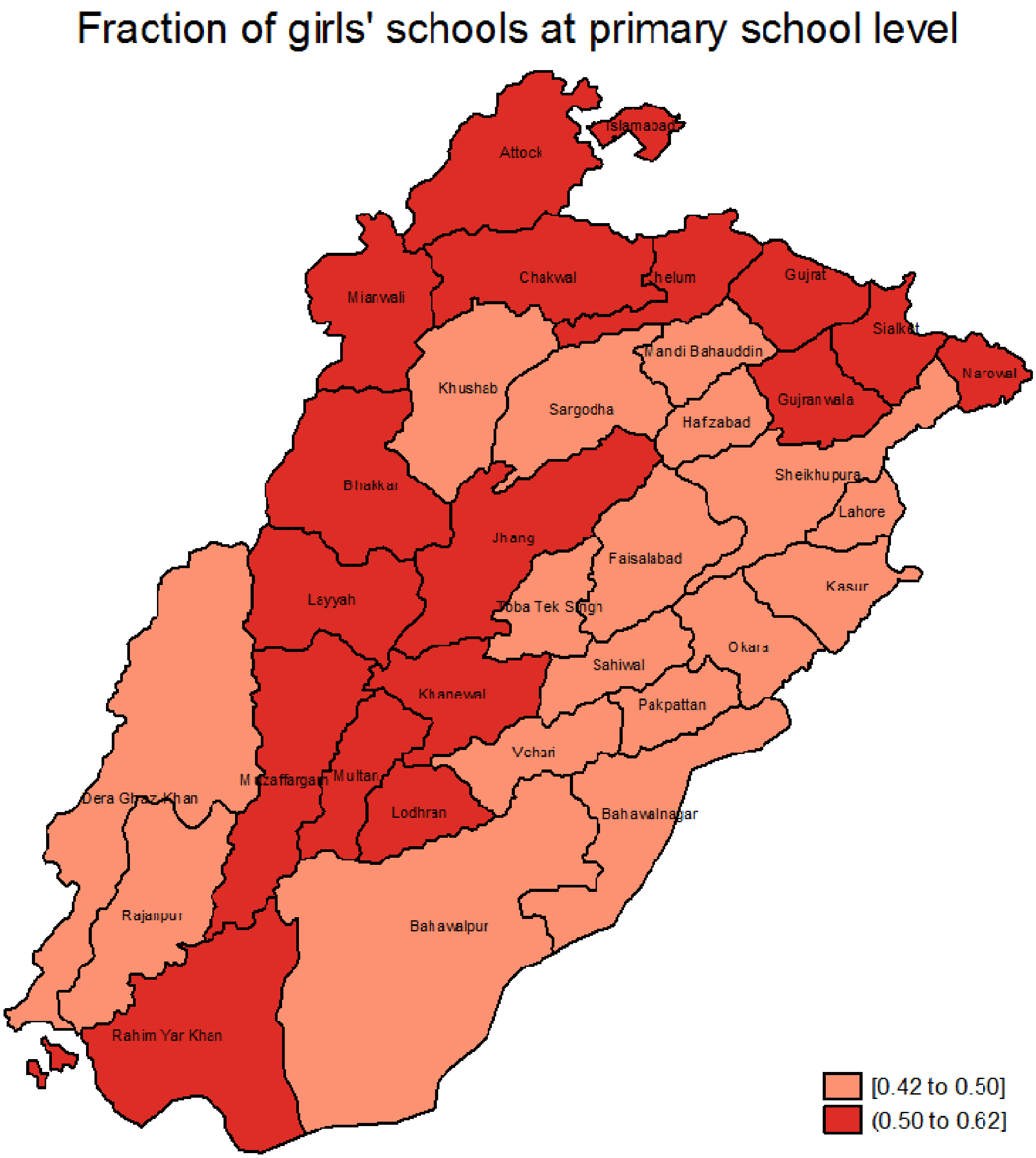


Figure 15: Fraction of girls' schools at the middle school level

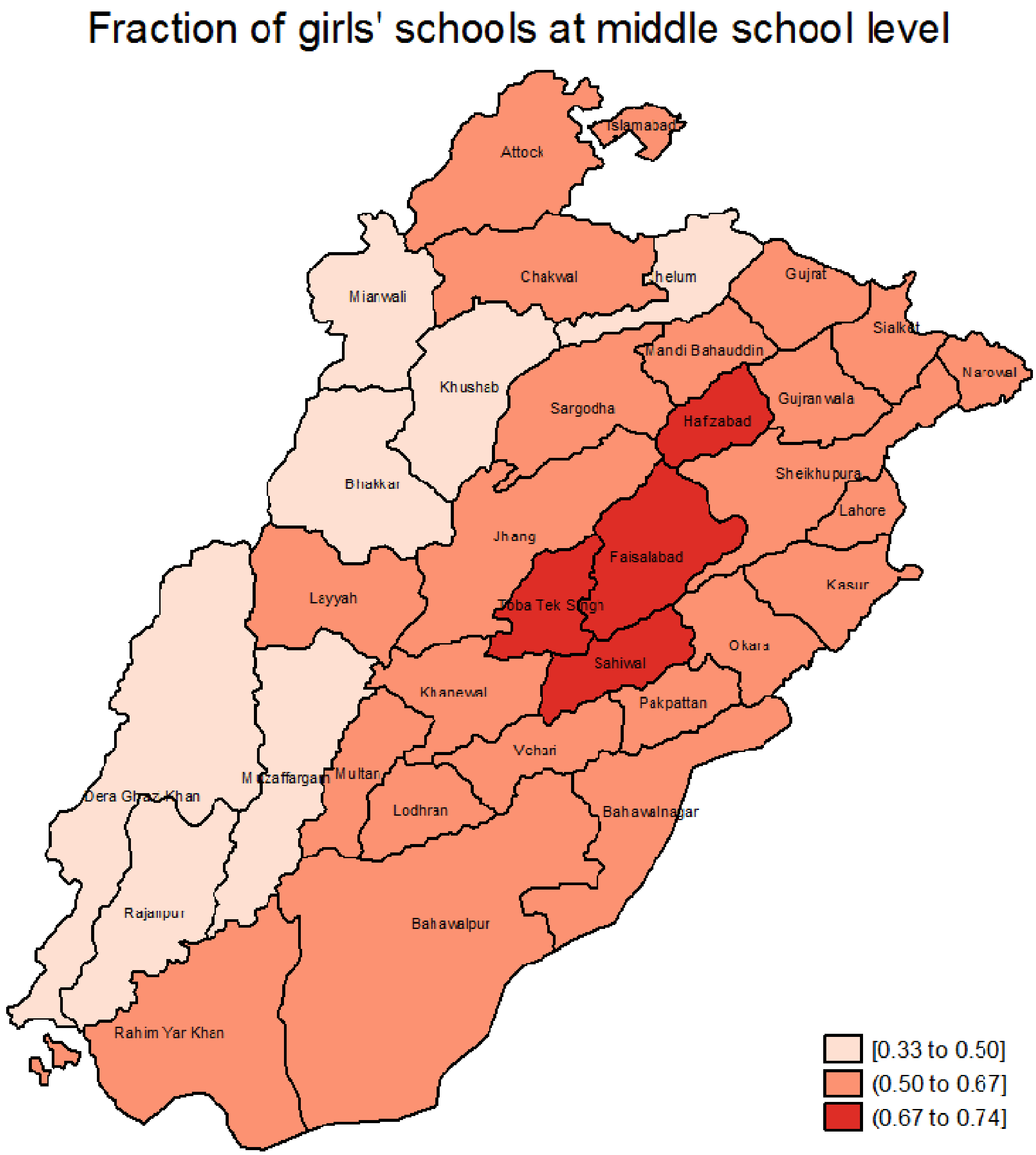
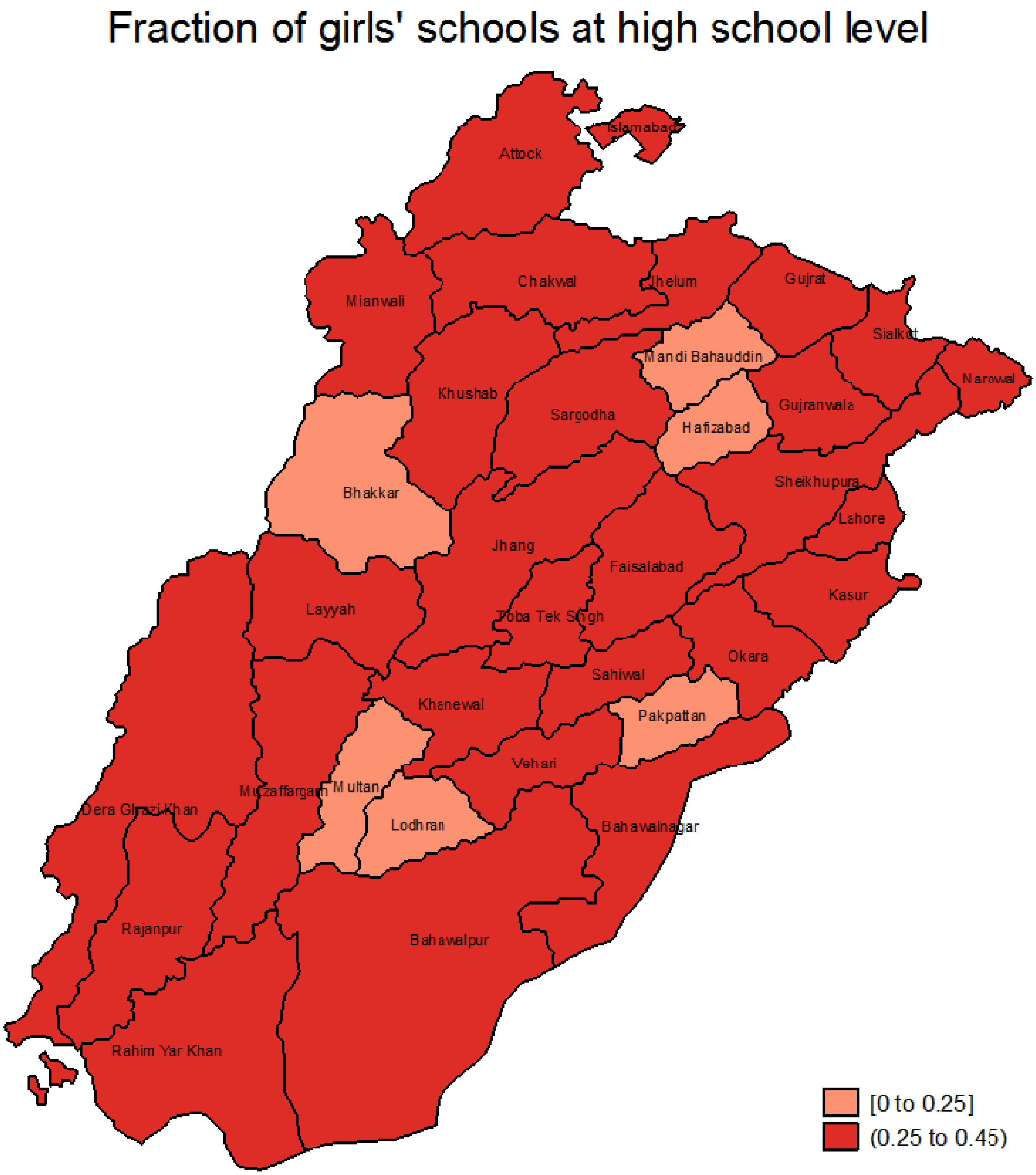


Figure 16: Fraction of girls' schools at the high school level



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